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EXPERIMENTAL OBSERVATIONS UPON NORMAL MOTOR SUGGESTIBILITY.

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The present paper deals with the modifications which certain motor coordinations display under the influence of sensory stimulations of a specific character. The coordinations selected were all movements of localization. The results of the experiments seem to throw an interesting light upon the psychophysical conditions involved in such movements and they indicate the possible nature of some, at least, of the familiar phenomena of suggestion. There appears to be reason to hope that a practicable, even though rough, criterion of general constitutional, or organic, suggestibility may, perhaps, be established along the line of the methods here employed.

The experiments, which grew out of some observations upon the localizing of contact sensations, include, in addition to extensive tests upon the localizing movements of the left hand, when the skin of the right forearm is stimulated, two sets of tests upon eye movements, when these are employed to localize sounds and visual objects. Throughout the paper the emphasis falls purposely upon the qualitative analysis of the conditions involved and not upon the quantitative features of the situation. This statement is not to be interpreted as prejudicing the relia-

bility of such quantitative statistics as are reported. These are all regarded as trustworthy within the limits of the methods adopted. But had the quantitative problems been of fundamental interest, these methods would have been modified in several particulars. The investigation is being carried forward with other and more exact modes of procedure.[1]

The literature bearing most immediately upon the subject of the investigation may be classified under two heads. First, that relating to the effects upon the contractions of both voluntary and involuntary muscles of various psychical conditions and especially the effects of various forms of sensory stimuli. The literature under this head is too extensive to cite exhaustively. Illustrative, however, of the general field are Mosso's observations of the circulatory changes accompanying emotional excitement;[2] Féré's observations upon the shifting power of voluntary muscular contractions under the influence of agreeable and disagreeable stimuli;[3] Mitchell's, Lombard's and Bowditch's studies of the modifications which the knee-jerk manifests under stimulations of both sensory and motor character.[4] Summarizing the positive purport of all these investigations, so far as they bear upon the present experimentation, we may be said to know already that muscular contractions reflect in an extremely sensitive way, capable within limits of quantitative statement, the momentary conditions of the whole nervous system. The cases we shall have to study will be seen to constitute a specific set of instances under this general principle.

As a subordinate investigation under this general head may be mentioned an article by Münsterberg and Campbell upon

[1] I am under obligation to Professor James R. Angell for counsel and assistance throughout the course of the experimentation. My sincere thanks are also due the persons who so kindly served as subjects for me, to wit: Misses M. Graves, E. Bartlett, E. Pearce, J. Biggars; Messrs. J. R. Angell, E. H. Mahood, C. A. Woolsohn, W. C. Gore, J. H. Lathrop, L. D. Arnett, E. L. Kimball, R. Clark, M. Hall, R. C. Adams, W. A. McKeevor; Masters H. Pearce and T. Pearce.

[2] Mosso, Kreislauf d. Blutes im Mensch. Gehirn.

[3] Féré, Sensation et Mouvement.

[4] Mitchell, *Medical News* (Philadelphia), 1886 (Feb. 13 and 20).
Lombard, *Amer. Jour. Psy.*, I., p. 1.
Bowditch, *Boston Medical and Surgical Journal*, 1888 (May 31).

the motor power of ideas.[1] I have adopted in part of my experiments an apparatus similar in many particulars to that used by these writers and by a different method have confirmed what they sought to establish. By varying the method I have also secured new results, which afford considerable assistance in interpreting the Münsterberg-Campbell observations.

The second main group of papers in the relevant literature concerns the discriminative sensibility of the skin. In the tactual experiments I have adopted methods somewhat similar to those used by E. H. Weber and later by Volkmann, Henri, Judd and others.[2] Further than a general similarity of method our experiments have little, if anything, in common. Such occasional points as are significant for my investigation will be more conveniently referred to in connection with the report of the experiments.

The rapidly enlarging field of literature on the subject of suggestion has necessarily had a general influence in determining my attitude with reference to the interpretation of my observations. As most prominent, perhaps, in this respect may be mentioned Sidis' 'Psychology of Suggestion,' Schmidkunz's 'Psychologie der Suggestion' and Binet's 'La suggestibilité.'

Dermal-Motor Suggestibility.

The dermal experiments number over four thousand and were made upon six persons, four men and two women. The conditions were varied slightly from time to time, as will be pointed out later, but in general they were as follows. The subject, being seated, was required to bare his right arm to the elbow and place the same upon a low table or other support, palm upward, and in as comfortable a position as possible. The stimulations were distributed equally over the region of the mid-arm, the wrist and that near the elbow, being given in the order named. The subject's eyes being closed, a fairly vigorous pressure was made upon the arm with a blunt instrument

[1] Münsterberg and Campbell, Psych. Rev., I., p. 441. Cf. also the drastic criticism of this paper by Nichols, *Phil. Rev.*, IV., p. 174.

[2] Henri, L'Année Psychol., II., p. 295. Also, Die Raumwahrnehmungen des Tastsinnes. This monograph contains an excellent bibliography of the subject. Judd, *Philos. Studien*, XII., p. 409.

and the subject was required immediately to touch as near the same point as possible with a similar instrument held in his left hand. He was allowed only one trial, *i. e.*, the point where he first touched was the one noted, and the error both in amount and direction was recorded. The normal displacement, *i. e.*, the average error when a single stimulus was localized, was first determined. To determine this ten tests were made with a single stimulus in each of the three regions of the arm named—the stimulations being given first on mid-arm, then on wrist, then near elbow, the process being repeated ten times. Then the same process was repeated with the addition of a second stimulus 6.5 cm. below the first, given immediately following, or in the case of some subjects simultaneously with the one to be localized.[1] Thus a series of ninety tests was completed. The experiments at each sitting usually occupied one hour, during which time from ninety to one hundred tests were made.

Various simple devices were employed for giving the stimulations. In the majority of the experiments an ordinary pair of æsthesiometric compasses was used, the points of which are spherical and about a millimeter in diameter. The points are likely to set up slight temperature sensations in addition to the touch sensation and in some of the series of tests wooden points were substituted. For the purpose of the investigation this is really a matter of no great consequence.

In making up the tables, it will be observed, I have disregarded errors made to right and left of the point to be localized. These would only complicate the situation. When the localization was exactly right or left the error is regarded as zero; if the localization was down or up *and* to right or left, the error down or up only is considered. These errors to right and left have apparently no uniformity and I could not see that they were materially altered when the second stimulus was given. The amount of such lateral errors varied from zero to about twelve millimeters, but was generally nearer the former figure.[2]

[1] This distance is considerably in excess of that where the two points are felt as one by most observers.

[2] The practical justification of this procedure is supported by Lewy's investigations—*Zeitsch. für Psychol. u. Physiol. d. Sinnesorg.*, VIII., p. 231. He found these errors which I disregard as few in number and insignificant.

Only two of the subjects were aware of the purpose of the experiments. They were instructed to attempt as much as possible to disregard the second stimulus, *i. e.*, not to allow it, consciously, to influence them in either direction. In other words, they were required to make the reaction in as reflex a manner as possible. The subjects who did not know the object of the experiments were told that the purpose of the second stimulus was to test the effect of distraction, to see if they could localize as well with as without a second stimulus. When questioned after all of the experiments had been completed, these subjects were still totally ignorant of the real purpose as well as the *result* of the experiments. All the subjects agreed that, so far as they could judge, the only effect of the second stimulus had been to render a trifle more difficult the identification of the point which they sought to touch. In order to test the accuracy of this testimony I had one of the subjects make a series of the tests on myself and I was unable to tell, until after I had examined the figures, what the nature of my own reactions had been. One subject had considerable difficulty in distinguishing the two stimuli as two when they were about six centimeters apart and given near the elbow. It was interesting to observe, however, that even under such conditions his reactions were practically as accurate as when the two points were clearly distinguished. This serves to illustrate the essentially reflex nature of these reactions.

In the experiments which have been recorded in the literature of this subject, the emphasis has, so far as I am aware, been placed upon the sensory side—upon the accuracy or inaccuracy of the sensory discrimination of the points stimulated. In my experiments this feature has been of secondary, even of incidental interest. I have worked upon the theory that every sensation and every image of a sensory stimulus has a more or less definite motor element of a kind tending to release movements of localization. In reflex action, for example, a motor impulse is somehow more or less definitely coordinated with a particular sensory stimulus. Disregarding the question of how the particular coordinations under discussion arise, it has been my purpose to determine the accuracy of

this motor impulse, and later, to determine how such impulses may be modified by other similar stimuli both successive and simultaneous. The subject, therefore, was not allowed to move the pointer after it had once touched the arm. This was frequently quite unsatisfactory to the subject and some subjects were anxious for further trial, believing they could do better. In many instances this was undoubtedly true—the subjects had a fairly definite idea of the direction and the amount of the error. But in many other instances they were quite satisfied with the result and in still other cases, when questioned or given permission to try again, their judgment was quite at fault. This last was particularly true if the first error was very small. On the whole, I think the tabulated results will show that the average errors in localization made by using this method are not very much greater than the errors recorded by others, who have allowed the subject to keep trying until satisfied; but it is not claimed that the figures represent in any degree the discriminative sensibility of the skin as a sense-organ. To be sure Henri comments on the great uncertainty attaching to our judgments of such localizing movements.[1] It is not entirely clear from his description how closely his conditions are comparable with mine. Apparently the localizations he refers to were made by pointing and not by effecting actual contact. This would certainly introduce an important difference. In any case the facts are as just stated. The difference in the accuracy of the localizations under the two modes of procedure seemed neither marked nor constant, and as the point to be investigated was the effect of the secondary stimulus upon the movement, rather than the sensitivity of the skin, the method adopted seemed entirely justifiable.

Results.

The first table presented (Table A.) shows the averages obtained from each subject. It will be noticed that with a single stimulus all of the subjects except one displace downward, *i. e.*, toward the wrist. V. Henri,[2] in his experiments, to which refer-

[1] Henri, Raumwahrnehmungen d. Tastsinnes, p. 114.
[2] 'L'Année Psychologique,' Vol. II., p. 177.

TABLE A.—TACTUAL.[1]

Subject.	Region of Arm.	Single Stimulus.				Suggestion Up.							Suggestion Down.						
		Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation.	Per cent. of Pos. Influence.	Per cent. of Neg. Influence.	Positive Influence in cm.	Neg. Influence in cm.	Tests.	Av. Error.	Direction.	M. Variation.	Per cent. of Pos. Influence.	Per cent. of Neg. Influence.	Positive Influence in cm.	Neg. Influence in cm.
A	Forearm.	60	.78	D	.57	70	.20	U	.77	74	26	.80	.02	70	1.08	D	.61	61	39	.46	.26
B	"	114	.72	D	.54	119	.77	U	.79	82	18	1.46		118	1.20	D	.82	68	32	.63	.23
C	"	57	.54	U	.42	58	1.06	U	.44	79	21	.98		58	1.86	D	.43	98	2	1.93	
D	"	96	.84	D	.71	111	.31	U	.72	84	16	1.11	.08	98	1.05	D	.64	64	36	.61	.31
E	"	131	.73	D	.51	156	1.51	U	.61	93	7	2.02	.01	163	1.02	D	.59	56	44	.79	.53
F	"	150	.39	D	.64	153	1.37	U	.80	90	10	1.77		159	.44	D	.58	44	56	.38	.38
G	"	236	.07	D	.66	221	1.54	U	.75	89	11	1.62		219	.57	D	.61	67	33	.69	.26

ence has already been made, found that his subjects usually displaced in the direction of a 'point of reference,' which they had in mind in making the localization. This point of reference was usually a bone or some prominent configuration of the surface of the skin. If, however, this point of reference was a considerable distance from the point touched, the error was usually made in the opposite direction. If the subjects use a point of reference, the fact noted by M. Henri appears to be quite reasonable, and I think the cause is to be found in the fact that the image of the point of reference itself has a localizing motor element and this merged with the corresponding element from the sensation or image of the point stimulated is quite sufficient to account for the error in that direction.[2] The use of reflective judgment, however, in my experiments was eliminated, and my subjects apparently did not have in mind any point of reference of the kind which M. Henri describes. How then shall we account for this almost uniform tendency to displace in the direction of the hand, even when the stimulus was given near the elbow? It is quite possible, of course, that the hand is always used, even unconsciously, as a point of reference when the localization is made on the forearm, but I think there is reason to believe that another factor plays a more conspicuous part. It is well known

[1] The figures showing the average error, mean variation, etc., are given in centimeters.

[2] The analogy between such a 'point of reference' and the zero point or focus of attention at the beginning of the auditory and visual experiments is manifest. See pp. 343 and 350.

that the nerve terminations in the skin are more numerous as we pass downward toward the extremities. Further, it has often been noted that in stimulating the skin we do not stimulate merely a point, but, on account of the tension of the skin, we stimulate an area. Inasmuch, then, as the nerve supply in such an area is greater in the lower half, it is quite conceivable that the attention should be focused not on its mathematical center, where the point was stimulated, but on a center of equilibrium, which must, by reason of the nerve distribution, fall in the lower half. President Hall, in explaining similar facts brought out in some of his investigations,[1] finds their origin in the genetic development of the race. Erroneous estimations of the sensations of movement involved in the localizations have also been adduced as responsible for the tendencies manifested.

Referring again to Table A, we find that when the second stimulus is given above the first, the error downward is changed into an error upward, and that in the single case where the natural error was up, the error is considerably increased when the second stimulus is given. On the other hand, when the second stimulus is given below the first, the natural error downward is considerably increased in the direction of the second stimulus. Table A contains a summary of all of the experiments. The first series of experiments does not show the same results as the later ones. In the first experiments the disposition to resist the influence of the second stimulus, and even to make an error in the opposite direction, was quite manifest. These results being included in the averages shown in Table A, have the effect of decreasing the average value of the influence of the second stimulus. It was my first intention, therefore, to leave out of consideration the results of the first series, regarding them as mere control experiments, but the gradual yielding to the suggestion as it is repeated time after time appears to me now as one of the most significant facts developed during the course of the experiments. In order, therefore, to show the change in the form of reaction, I present Table B, which shows all of the series of reactions obtained from a single subject. It will be

[1] See Hall and Donaldson, *Mind*, Vol. X., p. 557; also Hall and Motora, *Amer. Jour. of Psychology*, Vol. I., p. 72.

observed that when the second stimulus[1] is given above the first, it begins to have a consistent positive influence after the second series, and that when the second stimulus is given below the first, a consistent positive influence is not manifest until the middle of the fourth series. This table is fairly typical of the results derived from each subject. The subject is a graduate student of philosophy, accustomed to critical introspective analysis. He was not cognizant of the purpose of the experiments, and when questioned at the end was, like all the others, ignorant of the effect produced by the application of the second stimulus.

TABLE B.—TACTUAL.—SUBJECT G.[2]

Series.	Region of Arm.	Single Stimulus.				Suggestion Up.							Suggestion Down.						
		Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation.	Per cent. of Pos. Influence.	Per cent. of Neg. Influence.	Positive influence in cm.	Neg. Influence in cm.	Tests.	Av. Error.	Direction.	M. Variation.	Per cent. of Pos. Influence.	Per cent. of Neg. Influence.	Positive Influence in cm.	Neg. Influence in cm.
I	Mid-arm.	9	2.13	D	.92	9	.52	D	.80	89	11	1.61		9	.54	U	.44		100		2.67
	Wrist.	7	.20	U	.47	7	1.81	U	.41	100		1.61		7	2.20	U	.78		100		2.00
	Elbow.	9	1.82	D	.55	9	.06	D	.86	89	11	1.76		9	2.95	D	.65	89	11	1.13	
II	Mid-arm.	9	.33	U	.67	9	.03	U	.67	45	55		.30	9	.29	U	.33	45	55		.06
	Wrist.	8	.50	U	.20	8	2.60	U	.72	100		2.10		8	2.15	U	.67		100		1.65
	Elbow.	9	1.60	D	.87	8	.37	D	.37	75	25	1.23		8	3.61	D	.58	100		2.01	
III	Mid-arm.	10	.60	U	.30	10	3.01	U	.69	100		2.41		10	1.32	D	.66	100		1.92	
	Wrist.	9	1.03	D	.47	9	2.53	U	.90	100		1.50		16	.59	U	.62	7	93		1.62
	Elbow.	8	1.17	D	.52	9	3.43	U	.88	100		2.26		9	2.60	D	.42	100		1.43	
IV	Mid-arm.	7	1.17	D	.57	7	1.55	U	.71	100		2.72		9	1.10	D	.95	55	45		.07
	Wrist.	9	.88	D	.27	9	2.55	U	.77	100		3.43		8	.62	U	.63		100		1.50
	Elbow.	10	2.01	D	.67	10	1.54	U	.60	100		3.55		9	2.98	D	.72	90	10	.97	
V	Mid-arm.	8	.55	D	.37	9	1.98	U	.27	100		2.53		9	1.47	D	.53	78	22	.92	
	Wrist.	15	.56	U	.90	7	1.53	U	.60	86	14	.97		9	.31	U	.63	55	45	.05	
	Elbow.	8	.21	D	.46	9	1.44	U	.42	100		1.65		8	2.86	D	.43	100		2.65	
VI	Mid-arm.	8	1.15	D	.25	8	1.62	U	.52	100		2.77		8	2.03	D	.51	87	13	.88	
	Wrist.	10	.49	D	.39	10	2.00	U	.70	100		2.49		9	.63	D	.41	55	45	.14	
	Elbow.	9	1.22	D	.35	9	.55	U	.41	90		1.77		9	3.38	D	.70	100		2.16	

Variation in the intensity of the second stimulus affects the influence of the stimulus. Three different intensities were tried, —less, equal, and more intense than the first stimulus. I offer only a single illustration. When the second stimulus was less

[1] In the case of this subject, the two stimuli were given simultaneously throughout the experiment. Occasional observations have been made showing a tendency, when two such stimuli are felt as one point, to locate this midway between the two points actually touched. Henri's experiments do not confirm these reports. *Cf.* Henri, Raumwahrnehmungen d. Tastsinnes, p. 130.

[2] The average errors, mean variation, etc., are given in centimeters.

intense than the first, it produced a change in the reaction as compared with the normal of 1.51 cm.; when it was equal in intensity to the first, its effect was 2.17 cm.; and when more intense than the first, the effect was 3.00 cm. A change in the distance of the second stimulus from the first also affects the result. The greatest effect is produced when the second is distant from the first about 8 cm. in the case in which the second stimulus is given above the first. In the other case the greatest effect is produced at a distance of about 10 cm. Either increasing or diminishing this distance diminishes the influence of the second stimulus. The reason for the decrease in effect when the distance between the two points decreases is apparently simple and may be best explained according to the principle of the parallelogram of forces. But the other case is not so clear. I take it, however, that when the distance is greater than 10 cm., the sensory quality of the two stimuli becomes so very different, that attention is more definitely aroused and the compensatory tendencies, which were so manifest in the earlier experiments, are again more definitely engaged in the reaction. These tendencies are very manifest when one stimulus is given on the arm and the other on the palm of the hand—the sensory quality of the two regions being distinctly different. It appears quite probable, however, that even in these cases, if the experiments were continued long enough for the two qualities of sensation to become habitually associated, so that attention is no longer disturbed by the difference in quality, these compensatory tendencies would gradually disappear and we might thus finally say that the effect of the second stimulus is proportional to its distance from the first. The foregoing statement certainly agrees with, and possibly in a measure explains, the change in the direction of the error in the early stages of the experiments, to which reference has already been made.

Later Tactual Experiments.

For the purpose of comparing the results of the tactual with the auditory and visual experiments which are to follow, I have carried on another series of tactual experiments, in which I have endeavored to use a method similar to that which

I found it expedient to adopt in the case of the auditory and visual investigations. The subjects were two boys, aged respectively eight and seven years. They also served as subjects for the auditory and visual experiments, and hence the results from all three lines of experiments are comparable.

The method used was as follows. Beginning at a distance of 6.5 cm. from the junction of wrist and palm of hand, on the volar surface of the forearm, I marked off five lines parallel to each other, one centimeter apart, and perpendicular to the long axis of the arm. The stimulus to be localized was given on one of these lines, beginning with the one nearest the wrist and proceeding upward in regular order. Sufficient extra stimuli were introduced in irregular order to prevent the subject from forming too definite a conclusion as to the order. This precaution was observed in all the experiments. The same process as that just detailed was again repeated with the addition of a second stimulus given immediately after and 6 cm. below the first. The stimulus acted as a suggestion 'down.' The process was again repeated, but this time the second stimulus was given 6 cm. above the first and acted as a suggestion 'up.'

The accompanying table (C) represents three complete series for both subjects of ten tests each for each of the five standard points.

TABLE C.—TACTUAL.[1]

Series.	Subject.	Single Stimulus.				Suggestion Down.						Suggestion Up.						Per cent. of Suggestibility.	
		Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation	Positive Influence in cm.	Neg. Influence in cm.	Tests.	Av. Error.	Direction.	M. Variation.	Positive Influence in cm.	Neg. Influence in cm.		
I	T. P.	50	.4	D	.8	50	.8	D	.8	.4		50	.7	U	.9	1.1		68	
II		50	.8	D	.9	50	1.0	D	.6	.2		50	.4	U	.6	1.2		84	
III		50	.7	D	.6	50	1.4	D	.6	.7		50	1.8	U	.8	2.5		82	
																			78
I	H. P.	50	.9	U	.8	50	2.0	U	.6		1.1	50	2.4	U	2.0	1.5		51	
II		50	.7	U	.7	50	1.2	U	.9		.5	50	2.6	U	.8	1.9		61	
III		50	.6	U	.8	50	.2	U	.9	.4		50	1.6	U	.8	1.0		76	
																			63

[1] The average errors, mean variations, etc., are given in centimeters.

It will be noticed that the second subject, H. P., contradicts what I have previously said as to the direction of the normal error downward.[1] Again, in the first series, when suggestion 'down' is given, *i. e.*, a suggestion opposed to his normal tendency, he reacts against the suggestion in the opposite direction. This contradiction is decreased in the second series and the suggestion is fully effective in the third series. On the other hand, it is interesting to observe that the suggestion in the direction of his normal tendency, *i. e.*, up, decreases in force from the first to the third series. I shall have occasion to note and comment upon similar variations in the other experiments. The fact that such variations are constant under such diverse conditions would appear to have considerable significance.

In order to portray graphically the change in the character of the reactions, as well as the other features of the experiments, I have constructed curves representing each of the three series for the subject H. P. The points on the ordinate, viz. 7, 8, 9, 10, 11, represent the points on the forearm where the stimuli were given. These were respectively 7, 8, 9, 10 and 11 cm. distant from the juncture of wrist and palm. The figures on the abscissa represent the error in localization measured in millimeters. The general downward tendency of all of these curves is particularly remarkable, inasmuch as it shows that the absolute error in localization decreases as we proceed upward toward the elbow. This is probably due to a variation in the force of some motor element, which is not clearly apparent. There was no reason to suspect any peculiarity in the sensory discrimination for the region and this motor factor is seemingly the only direction in which an explanation could be found.

Summary of Results.

I. In the localization of single points stimulated on the forearm there is a general tendency to displace downward, *i. e.*, in the direction of the hand.

[1] I could discover nothing in the conditions of the experiment which would account for this contradiction. Outside of the possibility of the subject's using a different 'point of reference,' which I think improbable, I know of no way to account for the contradiction, except by reference to the general character of the subject.

Subject H. P. (Tactual)	Fig. 1.	Fig. 2.	Fig. 3.
—— Single Stimulus,	7.7	7.2	8.0
- - - - Suggestion Down,	6.4	9.8	9.1
..... Suggestion Up,	10.5	8.2	8.6
Average Suggestibility,	51%		76%

II. If a second stimulus is given *above* the first and the subject is required to localize the first stimulus, the normal error downward at the outset is *increased;* if the second stimulus is given *below* the first and the subject is required to localize the latter the normal error downward is at the outset *diminished.*

III. But, after a time, varying with each subject, the tendency just mentioned is reversed. A second stimulus below the first increases the normal tendency to displace downward and a second stimulus above the first changes the tendency to displace downward into a tendency to displace upward, *i. e.*, in the direction of the second stimulus. This second tendency is apparently permanent, so long as the stimulations are repeated. Whether reversion to the original tendency would ever occur cannot be said.

IV. The absolute effect of the second stimulus when given below is less than when given above the point to be localized. In other words, a suggestion to increase the normal tendency to error is less effective than a suggestion to reverse the nature of the error.[1]

V. The distraction of attention produced by the second stimulus is shown by a slight increase in the average variation. This increase is greater when the suggestion is 'up,' *i. e.*, in the direction contrary to natural displacements.

VI. The effect of the second stimulus is proportional to its intensity relative to the first.

VII. The greatest effect of the second stimulus results when it is given at a distance of eight to ten centimeters from the first.

Auditory.

The second series of experiments represents an effort to determine the influence of a sound, when the attempt is made to locate another immediately preceding sound of like character. The subjects used were eleven in number, seven men, two women and two boys. The two boys and three of the men also served for the visual experiments, thus affording some basis for comparison.

[1] This principle will be slightly modified in connection with the auditory and visual tests.

The apparatus employed in the experiments was constructed as follows: A semicircular table, six and a half feet in diameter and three and one-third feet high, bore on its outer circumference and extending above it to a height of six inches, sheets of white cardboard, on which were drawn the degrees of a circle. Back of this cardboard was a black curtain extending entirely around the outer circumference of the table and elevated two feet above it, thus concealing entirely the movements of the operator from the subject, as well as affording a homogeneous background. The subject is seated in the center of the circle, of which the table is a part, so that his eyes are equidistant from all points on the graduated cardboard, behind which the sounds are to be made. The sound, a short, sharp noise, is made with a metallic clicker commonly used in practicing the Morse telegraphic code. The eyes of the subject, at a given signal, are fixed upon a cross placed just above the zero point[1] in front of him and in the center of the scale. After two seconds a click is given at some point behind the graduated cardboard in the right hand quadrant. The subject immediately follows the impulse to turn his eyes in the direction of the sound heard and calls the number below the point upon which his eyes rest. In the first experiments I selected ten definite points at which the sounds were always given. Later, I found it more expedient to reduce the number to five. After a single stimulus was given at each of these points, the same series was repeated, but another click was made at the subject's left, usually thirty degrees from the first click.

With the first group of subjects used, I unfortunately adopted the plan of giving the distracting stimulus first, and the stimulus to be localized last. This was done for the purpose of reducing the variable error to the minimum. Under these conditions, suggestion to the right, after the first few tests, is always effective, but in the majority of cases the suggestion to the left, *i. e.*, toward the center, produces contradictory results—the localization is more accurate than with a single stimulus. The reason is plain. The attention is first carried to the distracting stimulus. But this point is nearer the point to be localized than

[1] See note on p. 335.

the zero point from which the attention should start. Hence the motor power of the distracting stimulus is *subtracted* from the motor power of the zero point, whereas, if the distracting stimulus is given after the stimulus to be localized, its motor power is *added* to the motor power of the zero point. With the second group of subjects, therefore, I adopted the latter method of procedure. The five points, which were to localized, were as follows: 30, 40, 50, 59 and 69 degrees. The sounds were usually given at these points in the order named, only such variation and introduction of extra stimuli being employed as were thought necessary to prevent the subject from being sure of the relative direction and distance of the succeeding sounds. None of the subjects were aware of the fact that constant points were selected, nor were they acquainted with the nature of the influence which the second stimulus was expected to exert. On being questioned at the close of the last series of experiments, the eleven subjects were somewhat divided in their opinion as to the effect of the second stimulus. One was aware of a tendency to follow the second sound; others thought it would probably have an opposite effect. But the majority thought that its only effect was to make the reaction somewhat less accurate. These impressions evidently resemble those reported in the tactual experiments.

Results.

Turning to the results themselves, I present Table D. This represents all of the tests with the second group of subjects, except a few made to determine the effect of variation as regards the distance of the stimuli apart, their relative intensity, etc. Reading through, for example, the first series of tests obtained from H., a man, we find that in 50 tests, when a single stimulus was given at the five points already named, he displaced the sound on the average 4.6 degrees to the left, *i. e.*, toward the center or zero point from which his eyes started. The mean variation for this series was 2.7 degrees. Again, in 50 tests, when a second stimulus was given 30 degrees to the left of the first, he displaced the first stimulus on the average 8.8 degrees to the left, showing that the second or suggesting stimulus has had a positive effect of 4.2 degrees on the average for each time

TABLE D.—AUDITORY.[1]

Series.	Subject.	Single Stimulus.				Suggestion to Left.					Suggestion to Right.					Per cent of Suggestibility.	
		Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation.	Positive Influence.	Tests.	Av. Error.	Direction.	M. Variation.	Positive Influence.		
I	H	50	4.6	L	2.7	50	8.8	L	3.6	4.2	50	1.3	L	3.5	3.3	69	
II	H	50	3.4	L	3.2	50	8.5	L	3.3	5.1	50	2.8	L	4.9	.6	66	
III	H	50	14.1	L	2.9	50	16.0	L	3.1	1.9	50	.8	L	4.5	13.3	84	
																	73
I	A	50	12.5	L	4.5	50	15.6	L	3.1	3.1	50	11.7	L	3.8	.8	70	
II	A	50	16.6	L	2.9	50	17.1	L	3.4	.5	50	6.9	L	4.2	9.7	72	
III	A	50	14.1	L	2.9	50	16.0	L	2.1	1.9	50	.8	L	4.5	13.3	84	
																	75
I	M	50	5.2	L	6.6	50	8.3	L	3.0	3.1	50	3.3	L	4.5	1.9	69	
II	M	50	5.8	L	3.4	50	9.0	L	3.5	3.2	50	1.3	L	5.3	4.5	73	
III	M	50	6.2	L	3.9	50	10.1	L	3.9	3.9	50	2.0	L	3.0	3.2	86	
																	76
I	T. P.	50	2.0	L	4.9	50	8.8	L	7.4	6.8	50	2.1	L	5.9	0	64	
II	T. P.	50	3.3	L	5.3	50	7.2	L	4.3	3.9	50	.3	R	6.8	3.6	72	
III	T. P.	50	2.6	L	4.9	50	5.5	L	5.9	2.9	50	2.8	R	6.9	5.4	62	
																	66
I	H. P.	50	5.1	L	4.3	50	6.3	L	5.0	1.2	50	.9	L	6.1	4.2	63	
II	H. P.	50	3.0	L	6.5	50	6.8	L	6.2	3.8	50	1.1	L	6.4	1.9	64	
III	H. P.	50	4.3	L	3.1	50	7.8	L	5.1	3.5	50	2.2	R	7.3	6.5	72	
																	66

it was given; mean variation 3.6 degrees. Similarly, in 50 tests, when the suggestion was to the right, there was a displacement of only 1.3 degrees to the left, showing a positive effect of 3.3 degrees for each suggestion, the mean variation with suggestion to right being 3.5 degrees. From this series I have calculated that H. showed a suggestibility of 69 per cent. This calculation was made in the following way: The normal error for a single stimulus at a particular point was determined by averaging the individual errors made at that point. This normal error was then compared with *each* error made at the same point under the influence of suggestion. If this suggestion is to the left and the error is now greater than the normal, it is considered that the suggestion has operated; if the suggestion is to the right and the error is now less than the normal, or if the usual error to the left has become an error to the right, *i. e.*, in the direction of the second stimulus, the suggestion is again consid-

[1] The figures showing average error, mean variation, etc., represent the degrees of a circle.

ered to have operated. Thus, comparing the number of times the suggestion was given with the number of times it proved effective, we secure an index of the suggestibility of the individual, at least for this particular kind of suggestion.

Turning now to the table (D) as a whole, we find that without exception all of the subjects, when a single stimulus is given, displace it toward the left, *i. e.*, in the direction of the center upon which the subject's eyes have been fixed. For the sake of comparison, I made a few experiments in the left quadrant and in this case the error is uniformly to the right—again toward the center. Binet, expressing a familiar view, has said that every image has a motor element, and Münsterberg and Campbell have made experiments, to which reference has already been made, showing, as they maintain, the 'motor power of ideas.' The fact just noted appears to sustain the theory of both of these contentions. It certainly appears insufficient to say that the inertia of the ocular apparatus is wholly responsible for this falling short in the localization of sound under the conditions described, though it is quite possible that this factor, together with the increased muscular effort necessary to turn the eyes toward the periphery, is partly responsible for the nature and the size of the error. It would appear, however, if Münsterberg and Campbell are right in attributing a certain motor power to the idea of the object from which the eye starts in its movement, that this motor power ought to be, under the same conditions, a constant quantity. At first glance the more detailed results of my experiments appear to contradict this theory. But I believe that this apparent contradiction is really a further confirmation. The detailed results show that the size of the error to the left increases with the distance from the center. This increase in the size of the error is not due to increase in the motor power of the original object of attention. But besides the increased muscular exertion made necessary by increased distance,[1] which might partially account for the change, there is also to be considered the increase in the number of ob-

[1] In a few exceptional cases the error decreased after the stimulus passed 45 degrees. This is possibly explained by the fact that the head was turned in that direction and the proportion of muscular effort necessary to turn the eyes was thus decreased.

jects intervening between the zero point and the point from which the sound arises. Each of these points, when imaged upon the retina, has its own motor power. This fact was well illustrated in a difficulty which I encountered with my apparatus as first constructed. The numbers were written by " fives " upon the graduated cardboard, and the line representing each fifth degree was slightly elevated above the intervening ones. I found that the subject almost invariably localized the sound at one of these prominent points and consequently I altered the apparatus, so that all the lines representing the degrees were equally prominent and were extended twelve inches upon the table and there numbered. Thus the subject was required first to select the line and then to trace it to the end, before he could determine the number which it represented.

Another interesting fact is revealed by examination of Table D. There is an evident tendency in the three series of tests for the positive influence of the suggestion, when given to the left, to decrease as the experiment proceeded from Series I. to III.; whereas, on the other hand, there is an equally evident tendency towards an increase in positive influence when the suggestion is made toward the right. When it is remembered that ' toward the left ' is the normal tendency to error and that in connection with the tactual experiments I said that a suggestion opposed to a normal tendency was more effective than the reverse, the real significance of the fact just pointed out becomes more apparent. We shall later find the same fact manifest in the visual experiments.

There is usually a slight increase in the mean variation when an interrupting suggestion is given, and as a rule the increase is greater when the suggestion is toward the right, *i. e.*, opposed to the normal tendency. All of the subjects except one show an increase in suggestibility from Series I. to III. The tendency to vacillation is sometimes very great at the first sitting and this tendency sometimes continued throughout an entire series of experiments, which usually occupied two or three days. The exception noted above is due doubtless to the conditions under which the third series of experiments was made. They were more hurried than usual and outside disturbances were more annoying.

In order to represent graphically the facts, which are brought out less clearly by the tabular statement just presented, I present some curves showing the progress of the experiments with a single subject, these curves being fairly typical of the results obtained from each of the subjects. In Figs. 4–6 are shown three curves. The points at which the stimuli were given are represented on the ordinate as 30, 40, 50, 59, 69, these figures representing degrees of a circle, counting from the center of the table, where the subject's eyes are fixed upon the zero point, to the right. The degrees of error in localization are represented on the abscissa as 5, 10, 15, 20, etc. The unbroken line represents the curve of normal error when a single stimulus is given; the broken line represents the curve of error when the suggestion is made toward the left; and the dotted line represents the error when the suggestion is made toward the right.

It will be noted that in the three series the normal error remains relatively about the same, the distance between the normal curve and the curve of suggestion to right increases. The general downward tendency of all of the curves is manifest.

Visual.

The problem here is to determine the effect of a second visual stimulus upon the localization of an object seen in peripheral vision. The object to be localized as well as the object used for suggestion was a piece of white paper 1 cm. square, pasted on a black cardboard 2 cm. square. These squares were attached to wooden rods, twelve inches long, which were arranged to slide noiselessly in grooves provided for the purpose, upon the outer circumference of the same table described in connection with the auditory experiments. The operator stood behind the screen and by pushing up one of the rods caused the visual stimulus to appear just above the edge of the semicircle. The stimulus appeared against the black cloth screen, which shielded the experimenter from the view of the subject. The points selected for giving the stimulus were 30, 40, 50, 59 and 69 degrees, the same as in the auditory experiments. After some preliminary experiments, 15 degrees was

MEAN VARIATION.

Subject A (Auditory). Fig. 4.

——— Single Stimulus, 4.5 deg.
– – – – Suggestion to left, 3.1 "
. . . . . Suggestion to right, 3.8 "
Average Suggestibility, 70%

Fig. 5.
2.9 deg.
3.4 "
4.2 "
72%

Fig. 6.
2.9 deg.
3.1 "
4.5 "
84%

selected as the most convenient distance to maintain between the two stimuli. The stimulus to be located appeared first and the suggesting stimulus immediately thereafter. The conditions observed in the auditory experiments were maintained as nearly as possible in the visual tests. A square similar to that to be located was placed permanently above the cardboard at zero in the center of the table. At a given signal the subject fixated the square.[1] Two seconds thereafter a stimulus appeared at 30 degrees, continuing in the field of vision not more than two seconds. Immediately after its disappearance the subject followed the impulse to turn his eyes in the direction where the object appeared. He noted the line nearest the point where the object seemed to be, followed the line down upon the table and reported the number corresponding to it. After stimuli had thus appeared at each of the five points already named, the process was repeated in the same order with the addition of suggestions to the left, *i. e.*, immediately after the first stimulus was given at 30 degrees, a second stimulus appeared at 15 degrees, etc. The subject had been previously instructed not to move his eyes, until after the disappearance of the second stimulus. After suggestions to the left had been given in connection with each of the five fixed points, the same process was again repeated, save that the suggestions were now made to the right. Except with one subject the start was always made at 30 degrees and the series progressed regularly to 69 degrees outward. In the case of the exception just noted, suggestion to the right, under these conditions, had the effect contradictory to that usually produced in the case of the other subjects. But if, after reaching 69 degrees when giving suggestion to the left, I reversed the order and gave suggestion to the right, working from 69 degrees to 30 degrees, the results compared very favorably with those derived from the other subjects. I can attribute this result only to individual peculiarity. If translated into terms of ordinary life, it would seem to indicate that the subject is almost abnormally opposed to innovations, that it is with great difficulty he is influenced to alter his habits, etc. I questioned him concerning these matters at

[1] See note on p. 335.

the close of the experiments, and his account of his feeling with reference to such things agrees exactly with such a presupposition. It was interesting to observe, however, that even in the case of this subject, before the end of the experiments, the influence of the suggestion to the right, *i. e.*, opposed to his normal tendency, became much greater than the influence of suggestion to the left, even showing some abnormality here again. Thus we see that though a suggestion opposed to the normal tendency may be less effective in the beginning than a suggestion which agrees with a normal tendency, ultimately the principle which I have already enunciated obtains, even in such abnormal cases.

Results.

To show the results of all the visual experiments at a glance, I present Table E. The subjects are the same as those used in the auditory experiments, the conditions were the same as nearly

Table E.—Visual.[1]

Series.	Subject.	Single Stimulus.				Suggestion to Left.						Suggestion to Right.						Per cent. of Suggestibility.	
		Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation.	Positive Influence in cm.	Neg. Influence in cm.	Tests.	Av. Error.	Direction.	M. Variation	Positive Influence in cm.	Neg. Influence in cm.		
I	H.	50	.6	L	1.3	50	3.0	L	1.5	2.4		50	0		1.6	.6		61	
II		50	.4	L	1.4	50	1.6	L	1.1	1.2		50	.8	R	1.3	1.2		72	
III		50	.5	R	.8	50	.2	L	.7	.7		50	.3	R	1.2		.2	54	62
I	A.	50	4.0	L	2.2	50	5.6	L	1.5	1.6		50	6.5	L	1.7		2.5	47	
II		50	5.0	L	1.3	50	6.5	L	1.2	1.5		50	5.3	L	2.4		.3	63	
III		50	4.5	L	1.1	50	5.2	L	1.0	.7		50	1.7	L	2.4	2.8		71	
IV		50	3.8	L	.9	50	1.6	L	1.1	.8		50	1.6	L	1.8	2.2		70	63
I	M.	50	5.2	L	1.3	50	7.4	L	1.4	2.2		50	5.0	L	1.5	.2		66	
II		50	5.1	L	2.0	50	6.1	L	1.7	1.0		50	3.8	L	2.3	1.3		69	
III		50	4.5	L	1.6	50	5.1	L	2.0	.6		50	2.6	L	2.6	3.9		65	67
I	T. P.	100	2.5	L	2.0	100	5.8	L	2.9	3.3		100	.9	L	2.8	1.5		80	
II		50	2.5	L	1.9	50	4.7	L	1.9	2.2		50	1.2	R	2.6	3.7		79	
III		50	.7	L	1.9	50	3.6	L	2.3	2.9		50	.9	R	2.3	1.6		76	78
I	H. P.	100	6.0	L	1.7	100	6.6	L	2.6	.6		100	4.6	L	2.0	1.4		54	
II		50	2.9	L	1.5	50	3.2	L	1.7	.3		50	1.7	L	1.4	1.2		51	
III		50	2.3	L	1.5	50	.9	L	3.3		1.4	50	.4	L	2.5	1.9		49	55

[1] Figures showing error, mean variation, etc., represent degrees of a circle.

as possible, and the results are of a remarkably similar character. As might be expected, the absolute error is less in the case of the visual tests, as is also the mean variation. The percentage of suggestibility is less in the case of the visual than in the case of the auditory series. This may be accounted for partly by the fact that the second stimulus in the visual experiments was only 15 degrees distant from the first, while in the case of the auditory experiments the distance between stimuli was 30 degrees. Taking into consideration the fact, which was established, viz., that the effect of the second stimulus varies with its distance from the first and also with its relative intensity, the difference in the percentage of suggestibility is not at all remarkable. It will be remembered that the absolute distances selected for the two groups of experiments were determined with reference to avoiding large variations in the localizations.

Showing the results of the visual experiments in a more graphic manner, I present some curves constructed similarly to those already shown for the auditory experiments. Figure 7 represents the curve of suggestibility of subject M., a man, as indicated by the first ten tests for each of the five standard points. Figures 8 and 9 represent curves of suggestibility for the same subject as indicated by the second and third series of ten tests each, for each of the five standard points. In comparing the auditory and visual curves it will be noticed that the scale on which they are constructed has been altered, the errors being somewhat exaggerated in the case of the visual.

Referring to the curves we see again the increase in error toward the periphery, shown by the gradual slope downward of all the curves; again we notice a gradual diminution in the power of suggestion, when given in the direction of the normal tendency; while the corresponding increase in power, when the suggestion is opposed to the normal tendency, is equally manifest.

The first effect of any change in the method of the experiments, *e.g.*, a change in the distance between the first and second stimulus, is to produce contradictory results. Attention is aroused and the subject reacts against the suggestion. I found it difficult, therefore, to get trustworthy results showing the effect of the change in distance between the two stimuli. To

MEAN VARIATION.

Subject M (Visual). Fig. 7. Fig. 8. Fig. 9.

——— Single stimulus, 1.3 2.0 1.6

– – – – Suggestion to left, 1.4 1.7 2.0

. . . . . Suggestion to right, 1.5 2.3 2.6

Average suggestibility, 66% 69% 65%

get perfectly reliable results it would be necessary to perform the same number of experiments for each of the distances selected, and, perhaps, to have experiments at all the distances selected at each sitting. This would have hindered the chief purpose of my experiments and would have involved more time than I had at my disposal. However, from a limited number of experiments, I think I am safe in saying that a change in the distance between the stimuli results in a corresponding change in the amount of error. For example, with one subject, I found that in fifty tests, with the two stimuli 20 degrees apart, the total effect of the second stimulus amounted to 16.8 degrees; while, in the same number of tests, with a distance of 15 degrees between stimuli, the total effect of the second stimulus amounted to 14.9 degrees. There would obviously be upper and lower limits for the operation of such a principle.

Summary of all Results.

1. In localizing a single stimulus, whether tactual, auditory or visual, an error is ordinarily made in the direction of the point upon which attention is directed when the stimulus is given. This error notably increases with the distance of the stimulus from the point of attention.

2. When a second stimulus of like character to the first is given, there is at first manifest a tendency to resist the suggestion thereby offered. But this tendency is diminished as the suggestion is repeated, and ultimately the suggestion becomes fully effective, causing the subject to err in the direction of the second stimulus.

3. The resistance, or the contradictory tendency, is most vigorous when the suggestion is made in a direction opposed to the normal tendency to error;[1] but ultimately a suggestion opposed to a normal tendency is more effective than in the reverse case.

4. Variation of intensity and distance of second stimulus, within certain limits, is followed by a corresponding variation in effect produced. The first effect, however, of any change in the method is to produce vacillating and contradictory results.

[1] This is true certainly in the auditory and visual experiments; less certainly in the case of the tactual.

It would evidently be difficult to hit upon a wholly satisfactory basis for comparing the suggestibility manifested by the three tracts of the nervous system investigated in the present experiments. Adopting the rough working principle of percentages employed in this paper, it appears that there is no exact correspondence among the several forms of tests in the case of any of the persons who served as subjects. Nevertheless the correspondence is close enough to be decidedly significant of considerable homogeneity in this respect throughout the nervous system. More notable, perhaps, is the fact that the same person shows the highest degree of suggestibility in each form of the experiment. Furthermore, if we compare the percentages of any two individuals in the different forms of the test, we shall find the ratios remarkably similar. The single exception to this statement occurs in the case of one of the subjects (T. P.) whose hearing was defective and whose results in the auditory series cannot fairly be used. For example, the percentages of one subject were for the visual tests 55 and for the auditory 66. For another subject the figures are 62 and 73. The proportion thus becomes 62:73::55:66. This gives us 4,092 = 4,015. The proportions are not all as nearly exact as this one, but the principle involved seems to hold throughout. So far, therefore, as the present observations permit a conclusion, they serve to indicate that conflicting stimuli provocative of localizing movements possess relatively equal values, whatever portions of the human nervous system they may effect. Given the degree of suggestibility of two individuals for one of the three tracts of the nervous system, we may reasonably infer relatively equal degrees of suggestibility for the other two tracts. This, in any event, is the obvious implication of my results, subject always to such modification as specific circumstances may involve.

Such a criterion of natural suggestibility seems to possess considerable advantage in point of exactness over that used by Binet in his tests upon school children.[1] He investigated the amount of modification he could introduce by suggestive questions into the judgments the children made of the lengths of visual lines. Such a method, while giving interesting indica-

[1] Binet, 'La suggestibilité,' p. 20.

tions of the general differences among children of different ages and education, clearly involves many shifting conditions of a purely social kind, which jeopardizes seriously the definiteness of the results. My method is not applicable immediately to large groups. But the results of it are certainly free from some important sources of error incidental to Binet's method. It is in this connection not without interest, perhaps, that, in the case of the two young boys who served for me, my results confirm very strikingly the opinion of all who know them well, as regards their relative sensitiveness to suggestion. In the present status of the subject we must frankly admit that any criterion is wholly tentative. I venture, however, to call attention to the fact that the parallelism is more than hypothetical between the conditions of my experiments and the conditions of an individual suddenly immersed in a new social and moral environment. Both are submitted to stimulations of whose exact purport they are often quite unaware. The first reaction is one of withdrawal and aversion. But under repeated exposure to the stimulus, without any explicit consciousness of what is occurring, they gradually yield and take on a new habit, which is the expression of a process that has hardly at any point involved reflective consciousness.[1]

If the theories of hypnotic suggestion, which have been couched in terms of inhibition, can give a really satisfactory account of the suppression of inhibitive associative processes, such facts as are reported in this paper would seem to show that the nervous system is entirely capable of executing the remaining activities, which have to be accounted for in explaining the effectiveness of such suggestion. This matter has been sufficiently discussed, as regards the psychophysical mechanism concerned, in the body of the paper and need not be again subjected to comment. The motor impulses intrinsic, in adult life at least, to sensations and images contain the clue to adequate explanation.

[1] In connection with reflective consciousness, attention may be directed to the process of the judgment involved in such experiments as these. In cases of this kind there is essentially a balancing of motor tendencies within the nervous organism and the resultant of these tendencies *is* the judgment. As a concrete instance of one form of relationship between judgments and motor activities it is not altogether devoid of interest.

FROM THE WELLESLEY COLLEGE PSYCHOLOGICAL LABORATORY.

The Perception of Sound Direction as a Conscious Process.

BY E. A. McC. GAMBLE.

This report deals with an attempt to analyze the auditory perception of direction. The results have led rather to the discounting than to the high appreciation of timbre, pitch, and intensity as conscious criteria of position.

Preliminary Discussion.

Let us assume, for the moment, that (1) timbre, pitch, and intensity, (2) reflex and semi-reflex movements of the head and eyes in the direction of a sound, and (3) cutaneous impressions about the ears, neck or scalp all act from time to time as factors in the localization-consciousness. If they do so act, they are factors of a very different kind. The consciousness of timbre, intensity, or pitch must serve as the link or pivot in an ordinary case of associative supplementing. Such localization finds its parallel in the gauging of distance by visual size or distinctness of outline. On the other hand, definite reflex movements in response to a sound must be genuine motor local signs. They find their parallel in the reflex jerk of hand or foot toward any irritated bit of skin. Timbre, intensity, and pitch are homogeneous criteria,[1] that is, they are position marks in the character of the sound itself, but they are not in themselves place-ideas. The after-consciousness of a reflex movement is a heterogeneous criterion, yet it is in itself a crude localization, a rudimentary tactual-motor idea of direction. Finally, if sounds are localized by 'touches' due to air-impact, auditory localization is, *ipso facto*, reduced to cutaneous.

[1] Titchener, 'Experimental Psychology,' Instructor's Manual, Qualitative, p. 357.

As a matter of fact, all primary criteria for localization are exceedingly elusive. Certain painstaking and trained observers maintain that a telephone-click correctly localized at different points does not seem to them to differ in timbre or in intensity or in any other way. Undifferentiated sounds seem to them to be instantly succeeded by differentiated ideas of place. Moreover, most observers note very rarely that they either move or 'want to move' when they hear the familiar sound of an experimental series. While practice diminishes the 'startling' effect of a sound, it notoriously increases the accuracy of localization. Apparently, just as a tap on the forehead may seem instantly to call up a picture of one's forehead or the word 'forehead' without any perception or memory of movement, so a telephone-click above one's head may evoke an image of the top of one's head and of the telephone held above it without any consciousness of head or of eye-movement.

Motor local signs doubtless play an all-important part in cutaneous localization in the days when the baby so assiduously explores with hands, feet and tongue the surface of his own small person. Yet we habitually localize that which touches us without attending to any group of sensations due to movement and perhaps without being conscious of any. It is hard to tell whether the motor local sign disappears from the content which introspection grasps through inattention and forgetfulness, as the act becomes habitual, or whether it ceases to come into consciousness at all. At any rate, it is conceivable, on the analogy of cutaneous localization, that motor local signs should for a time play an essential part in auditory localization and should then lapse from clear consciousness.

A serious objection may, however, be urged against this supposition. Auditory localization is decidedly inaccurate and is obviously improved by practice. Now, in so primitive and fumbling a process, motor local signs might be expected still to appear. The only answer to this objection is that the introspection of any process of localization is extraordinarily difficult, and that when practice has made the after-examination of auditory localizations easier, the motor sign has had time to lapse. The difficulty in introspection seems to be due to the fact that local-

ization, whether correct or incorrect, definite or indefinite, is so exceedingly rapid. The adult, when wide awake, does not leave for an instant any impression entirely unlocalized, that is, without any spatial associations. The explanation of this greed for spatial connection is presumably teleological. At any rate, even if a sound is assigned to a wide and vague area or is 'heard in two places at once,' some place-idea usually flashes into consciousness before one can catch it in the kindling.

We may, perhaps, assume that an observer with some training in psychological experiments will know, if in the long run he gauges position largely by timbre or intensity, or by 'involuntary' movement, or by cutaneous impressions. It cannot be forgotten, however, that an observer always tends to call a process which he himself cannot analyze 'elementary' or 'intuitive' or something of the sort.

In this report, the introspection of six subjects of some general training is taken in connection with the degree and the kind of error in their results. Apart from the aid of introspection, light upon the process of auditory localization has been sought in the effect of suggestion on the degree and direction of error. Laboratory exigencies compelled the employment of many untrained observers in these latter experiments and the massing of their results. The greater the effect of suggestion, the more likely is auditory localization to be an associative process, and the less likely is it to be the outgrowth of inevitable reflex movement. This we may surely assume on the basis of our general knowledge of the effect of suggestion.

A sharp distinction must be drawn between (1) the primary criteria of position, such as timbre, intensity, a 'touch' on the ear or the like, (2) the terms of localization, that is, the mental imagery of place or direction, and (3) the way in which the observer indicates position to the experimenter. However the direction of a sound is revealed to the observer, he may image the place or direction visually or in tactile terms or may name it. However he thinks of it in the first instance, he may either name it or point it out to the experimenter. We certainly tend to move our hands when we turn our heads, and in the same direction. Willing and ready pointing may, therefore, be an

indication of tactual-motor place-imagery and of a tendency to reflex movement of the head. The indication is, however, very insecure. With one of the six observers mentioned, ready pointing resulted simply from unusually definite visual imagery.

Apparatus and Method.

The sounds were all given at points on the surface of an imaginary sphere, having a radius of one half-meter and centering in the middle of the line connecting the observer's drum-membranes. The cardinal points were R and L, directly to the right and left in the 'auditory axis,' F and B, the points directly in front and behind in the same plane, U, the point overhead, 90° from the horizontal plane, and theoretically, D, the point 180° from U and under the observer's chair. The arcs limited by these points are called RF, LF, FU, and so on. Points on the horizontal and sagittal meridians are indicated by counting the number of degrees from R or L; points on the median meridian are counted from F or B. Thus, FU30 means 30° up from F on the median meridian. Directions are indicated in terms of opposite cardinal points. Thus, FB means backward, BF, forward, and so on.

In all the experiments, the sound was a telephone-click of fairly uniform intensity. The circuit was made and broken with a push-button two seconds after the spoken word 'Ready!' In the earlier experiments, the original Pierce and Münsterberg apparatus[1] was used with unimportant modifications. In the later experiments, the Titchener 'sound-cage' was employed. As adapted to our purposes, this apparatus consists of (1) an iron gas-pipe fixed in the ceiling, (2) a semi-circle of heavy brass wire, 102 cm. in diameter, rotating freely about the pipe as a vertical axis, and (3) a similar semi-circle just fitting within the larger, suspended from it, and rotating freely about a horizontal axis.[2] The vertical semi-circle is braced with a metal cross-bar. The telephone receiver is clamped to the center of the smaller semi-circle. Each semi-circle is provided

[1] Psychological Review, Vol. I., p. 464.

[2] Cf. Titchener, 'Experimental Psychology,' Student's Manual, Qualitative, p. 179. The apparatus is made by the Chicago Laboratory Supply and Scale Co.

with a disc graduated to hundredths and with a pointer. The readings give the position of the telephone in terms of the 'latitude and longitude' of the imaginary experimental sphere. The sound can be given exactly at any point except within a segment of about 72° in diameter which is cut out of the lower hemisphere by the chair and person of the observer. Moreover, one can instantly find the direction and degree of error if one moves the telephone to the point at which the sound is localized by the observer and takes the new readings. The apparatus is noiseless.

Of the six observers who came repeatedly and whose introspection has been especially noted, Cs. is an instructor in psychology, Ck. was laboratory assistant at the time the experiments were made upon her, D., P. and S. were senior students in a second year course in psychology, and G. is the writer.

The experiments fall into four groups: (1) Experiments with and without suggestion made upon unpracticed observers with the Harvard apparatus; (2) experiments of the same kind with the Titchener apparatus; (3) experiments on the observers, Cs., D. and G. with the Harvard apparatus; and (4) experiments on Ck., P., S. and G. with the Titchener 'cage.'

The details of method which are really important are: (1) the attempt to eliminate all anticipations of position, and (2) the pointing required of the six special observers. With the Pierce and Münsterberg apparatus, the range of positions from which the experimental series were made out included sixty-nine; with the Titchener apparatus it included one hundred and sixty-one. No observer had the slightest intimation of the points to be selected from these wide ranges. Only G. exactly knew the range of points from which the series were drawn. The earlier observers saw the Harvard apparatus. With the Titchener apparatus, however, the subjects were blindfolded,[1] and no one of them, except Ck. and G., had ever seen it. Under these conditions, it was necessary for all the observers except G. to indicate most positions by pointing. One complete series was

[1] The bandage was of black China silk, loosely folded; it extended merely to the temples, and was secured by narrow ribbons. It surely could not modify the sound impression.

taken with G., in which she simply pointed in the direction of the sound and avoided words, and another in which she stated position in terms of the apparatus readings in the series, all multiples of five. The other observers were allowed to correct their pointing in words. With the Harvard apparatus, unpracticed observers were permitted to indicate position in any way they pleased. Cs., D. and G., however, were required to point out the position of the sound first with the eyes shut and then with the eyes open.

These two circumstances taken together, (1) the elimination of expectation by the use of so many positions, and (2) the pointing, sufficiently account for the small number of right cases shown in the tables. They seem absurdly small as compared with the results of other experimenters. With an approximation to the conditions of Professor Angell and Dr. Fite and of Mr. Matsumato, however, our results approximate to theirs. Naturally, our observers often said that the sound came from a point far within the surface of the sphere. In these cases, the outer termination of the radius passing through the point (as nearly as we could guess at the line) was taken as the localization point. A given trial might not be repeated unless the subject professed inattention or complained of some indistinctness in the sound. This rule also tended to diminish the number of right cases. It was sometimes broken, however.

The questions put to the observers were: (1) How do you know where the sound is? (2) Do you seem to see the place or do you feel yourself move toward it or do you think it in words? Other questions were dropped as unduly suggestive.

Introspective Results.

Statements in regard to the terms of localization will first be considered, and second, testimony as to the primary criteria, the conscious clues to the place.

With G., visual imagery has been present from first to last in every series of experiments. The visual idea of place is deliberately translated into tactile terms, just as one pictures the relative position of objects in a dark room and then 'feels about' for them accordingly. Cs. and D. reported visual imag-

ery in nearly every case for which they gave any introspection. Cs.'s remarks on the required pointing show that it was secondary and deliberate. D.'s introspection is very scanty. No comment on the act of pointing can be found. Cs. and D. pictured some part of the 'cage' more often than G. did; G. 'saw' her own person, in the form of a vaguely outlined shadow, more often than Cs. and D. visualized themselves. Cs. and D. often pictured the cage without the telephone; G. almost invariably saw in a given position the telephone or a spot or streak of luminous color standing for the sound.[1]

Ck., on the other hand, showed a clear case of tactual-motor localization. She 'could have a clear picture of the room and the telephone if she wanted to, but the hearing of the sound did not call up the picture.' She 'simply heard and pointed, deliberately and not on impulse.' To this testimony she consistently adhered. P. and S. could give no lucid account of their imagery. At first, P. certainly had visual imagery, for she spoke repeatedly of a 'black marble' or 'ball of polished oak.' S. (although she also is a 'color hearer') seems to have localized rather more largely in words than did the other observers.

The great majority of the wholly unpracticed subjects saw something near their own persons—a line of some kind leading to their heads, a wooden ball, an ebony box, or the like. Two blind observers were tested. H. is a young woman who lost her sight by an accident at eight years old, and is now a teacher of the blind. She said that she 'saw the place in her mind' and that 'pointing was an expression of what was in her mind.' R. is a student at the Perkins Institute and has been blind from birth. She could give no clear account of her experiences. She said, however, that when she heard the sound, she 'pointed at once so as not to lose it.' Pointing seems, therefore, to have been deliberate.

[1] This observer shows a marked case of colored hearing. The color seen at first and most often was a sort of corn-color, the color of the click. Sometimes, however, pink, brown, white, green, or golden yellow would appear. The colors could not be anticipated. Of late, no color has been seen except the pale dull yellow of the sound. These phenomena are of interest in showing the highly visual character of the localization.

To turn to the primary criteria of localization. Cs. spoke in two cases out of 209 of a difference in intensity and once of a difference in timbre. D. spoke in six cases out of 248 of a difference in intensity; never, of a difference in timbre. G. knows that sounds given directly above her head are fainter with the Titchener apparatus than other sounds. It took her some time to discover this fact, but of late she has been able to use it successfully as a criterion. It must be noted that it was gleaned from the observation of sounds already localized with assurance. She has sometimes fancied that sounds given behind are 'thin' but makes mistakes in using this thinness (a visual character) as a criterion. Ck. noted no such criteria. P. said, late in the course of the experiments, that 'something in the sound' told her whether it was 'up or down,' 'right or left,' but not whether it was 'back or front.' She frankly remarked, however, that she thought she had 'once heard something of this sort in class.' S. said that sounds given in front and above were 'more explosive.' H. said that the sounds had an 'individuality due to their pitch' (timbre?). An unpracticed observer would occasionally allude to a difference in intensity or 'pitch.'

On the other hand, Cs. rather often (in fully 50 per cent. of her total number of cases) spoke of a touch, or a tickling or tingling somewhere on the scalp or on the inside of the ear. These pressure experiences often preceded her visualizations. The sound, however, was not always located on the same side as the skin impression. G. occasionally noted 'touches' on the head. She certainly feels the vibration when the sounds are given very far down and, therefore, very close to her body. D., Ck., S., H. and R. never noted such experiences. Other subjects, however, noted them not infrequently. Stress is laid on these experiences since Professor Angell and Dr. Fite say that, in their monaural experiments, they 'found no good evidence for supposing that cutaneous sensations played any part in the localizations.'[1]

Farther, Cs., G., D. and Ck. sometimes noted a tendency to involuntary head-movement. Ck. maintained that she had a general tendency to move her head toward the sound. G. did

[1] PSYCHOLOGICAL REVIEW, Vol. VIII., p. 246 (May, 1901).

actually move more than other observers, but usually did so quite unconsciously. Cs. noted a feeling of movement in the greatest number of specific instances—in only about 6 per cent. of her total number of cases, however. The only testimony which indicates automatic hand movement is P.'s. She said, "If the stimulus should act more slowly or by steps, perhaps I could introspect, but it all happens so fast. I hear the click and realize that I know where it is. At about the same instant my finger is at the place I hear it."

To summarize: The only observer explicitly to claim a primary criterion was Ck., a tactual-motor localizer. She maintained, as a general observation, that head-movement was 'the natural way to localize a sound,' but that she pointed to the place deliberately. On the other hand, P., who sometimes visualized and who noted differences in the quality of the sound in different positions, made automatic hand-movements, independent of the quality-feeling. Cs. imaged position in visual terms, but the visualization was often preceded or accompanied by cutaneous impressions and sometimes by a consciousness of head-movement. Of D. we know only that she visualized constantly and that she once or twice noticed involuntary head-movements. S. localized in part verbally. She insisted that she 'could not tell how she knew where the sound was.' G. has always visualized sounds. She explicitly maintains that there is ordinarily nothing in consciousness between the 'sight' of the sound and the developed picture of a spot of color or of the telephone in a certain position. One passes into the other as if figures about a central figure, already dimly seen, came swiftly through a mist. She has developed one intensity criterion by experience, and uses it when the sound looks like a streak through the whole median plane. She occasionally is guided by 'touches' and sometimes perhaps by head-movements.

We may anticipate in saying that P., G. and Ck. localized with comparative accuracy; that Cs. and D. in spite of practice localized with about the average accuracy of unpracticed observers; and that S. and, strange to say, the two blind observers, H. and R., fell far below this average.

Experimental Results.[1]

Figures will be given simply (1) to show one peculiarity which runs through all of our results, (2) to make clear some of the individual differences of the observers who were studied separately, and (3) to indicate the effect of suggestion upon the ordinary tendencies in localization.

In the first place, it would seem from our results that observers, at least in the beginning, tend to localize sounds given at B correctly and to localize all other sounds farther back than they are given.[2] From 32 unpracticed observers tested with the Titchener apparatus, the following massed results were obtained:

Table I.

SHOWING THE TENDENCY OF UNPRACTICED OBSERVERS TOWARD LOCALIZATION IN THE REAR.

Point.	Cases.	Right Cases per cent.	Direction of Error. Cases in per cents.			
			FB.		BF.	
			Under 72°.	Over 72°.	Under 72°.	Over 72°.
F	117	2	14	81		
FU 36	119	1	9	80		
FD 36	119	1	3	94		
B	120	24			57	3
BU 36	118	10			59	4
BD 36	118	14			54	1
U	52	12	37	52		
R	103	25	38		28	
RU 36	118	5	36	1	45	
RD 36	117	11	41		40	
L	104	22	49		15	
LU 36	118	8	43		34	
LD 36	119	7	45		31	
Total,	1442	11	23	23	29	1

The number of small errors in localizing points to the rear is undoubtedly increased by the difficulty of pointing backward with precision.

The backward tendency is quite as striking in the results of

[1] The following students served much as experimenters: Misses A. P. Cromack and C. M. Locke, 1900; D. Donner, 1901; H. B. Decker and M. B. Wood, 1902, and E. E. Pennell, M.A. 1901. Many other students took part as experimenters. Special acknowledgment is due to Miss M. C. Smith, laboratory assistant, for help in the later work.

[2] The same tendency appears in the Angell and Fite results, though not in so marked a degree.

the subjects tested with the Pierce and Münsterberg apparatus. Points are correctly localized at B and BU 30 more than twice as many times as at F and FU 30. Localization is more accurate in the whole of the backward hemisphere. The points most correctly localized are not due R and L, but RB 60 and LB 60. The following results (including only errors which could be counted in degrees upon one meridian) were obtained from about 60 unpracticed subjects:

TABLE II.

ALSO SHOWING THE TENDENCY OF UNPRACTICED SUBJECTS TOWARD LOCALIZATION IN THE REAR.

Meridian.	Cases.	Direction of Error.	Degree of Error. Cases in per cents. Under					
			30°.	60°.	90°.	120°.	150°.	180°.
Median.	733	FB	10	15	9	2	4	4
		BF	8	7	2	.1	.7	.5
Right horizontal.	729	FB	8	2	2	2	.7	
		BF	8	.7	.3	.1		.1
Left horizontal.	732	FB	7	2	1	.8		
		BF	6	2	.3		.1	

The majority of errors which could not be computed upon one meridian consisted in taking the arcs RF and LF for RU and LU, an illustration of the same backward tendency.

TABLE III.

SHOWING THE INDIVIDUAL DIFFERENCES OF THE SIX SPECIAL OBSERVERS.

Observers. / Münsterberg Apparatus.	Cases.	Right Cases per cent.	Direction of Error.			
Cs., eyes open.	205	9	Tendency : FB : BF :: 27 : 26			
Cs., eyes shut.	209	12	FB : BF :: 24 : 30			
D., eyes open.	248	11	FB : BF :: 19 : 27			
D., eyes shut.	198	10	FB : BF :: 18 : 29			
G., eyes open.	225	24	FB : BF :: 23 : 30			
G., eyes shut.	188	24	FB : BF :: 21 : 27			
Titchener Apparatus.			Cases in per cents.			
			FB.		BF.	
			Under 72°.	Over 72°.	Under 72°.	Over 72°.
Ck. } Pointing blind-folded.	441	18	24	13	24	3
P. } Pointing blind-folded.	448	22	31	5	19	7
S. } Pointing blind-folded.	426	6	39	23	19	.9
G. } Pointing blind-folded.	450	18	28	5	23	17
G. guessing at apparatus readings.	421	26	24	4	15	8

Table III., page 367, shows the comparative accuracy of the six special observers, and also their relative tendency to 'backward errors'.

The 'tendencies' FB and BF in the first part of the table are estimated by taking the percentage of errors FB and BF in relation to the number of cases given in front of the sagittal plane and behind it. With the Titchener apparatus, the points at which the sound was given were evenly divided between the hemispheres.

From these figures it would seem that the tendency to localize behind is not coupled with strong visualizing tendencies. From the fact that the tendency is so strong in the massed results of 90 unpracticed subjects it would seem that it is incident to the first systematic attempts at localizing sounds. It may be that the tendency has some teleological significance. The ears of an animal in a measure guard his rear. The tendency to turn vanishes as a sound becomes familiar. This is the only explanation which can here be offered. At any rate, the backward tendency is not due in any way to the position of the apparatus or of the experimenter. The apparatus was set up in two different rooms, and the direction in which the observers faced was, for a time, systematically altered to exclude constant sources of error.

To turn in the second place to the peculiarities of the six special observers: Cs., D., Ck., P., S. and G. It is hard to tell whether P. or G. is the more accurate localizer. G. is a very eye-minded observer, and is thus perhaps at a disadvantage in blindfolded pointing as compared with P. and Ck. It would seem from the first part of Table III. that a visualizing subject may be quite as accurate in pointing with closed eyes as in pointing with open eyes, provided he opens his eyes every moment or two and so gets his bearings. In the dark, however, the visualizer soon 'loses his place,' and his hand-movements resemble the random padding about of a baby who has not learned to 'use his hands.'

At any rate, the three most accurate observers all claim to localize immediately (that is, directly) in terms of hand-movement or visual imagery or head-movement, and not by any

character in the sound itself. G. has had more practice than the other observers. It is an odd confirmation of the automatism of her localizations that she places the sound more accurately when she is not paying very good attention.

It would be rash to infer that the imagery of localization affects its accuracy. Two of the more accurate observers localized in tactual terms and one visualized; of the less accurate observers, two visualized and one could give no clear account of herself.

The largest number of large errors were made by S.; the wildest and most anomalous errors were made by Cs. Cs. was the only one of the six who often confused the median and the sagittal arcs. The most perversely consistent errors were made by G., who habitually in her later work took B for F, BU 36 for U, and FD 36, a point practically in her lap, for some point in the arc BU. In the last (verbal) series she might have had 50 per cent. of right cases if she had not persisted in thinking of the horizontal plane as at the level of her neck, and R and L as straight out from the corners of her eyes, a natural error in an eye-minded subject. This stereotyping of certain tendencies may be another indication of automatic localization.

In the third and last place, the effect of suggestion must be considered. For a time, an attempt was made really to deceive the observers. They were flatly told that sounds would be given in a certain quadrant or hemisphere. A few of them detected the deception but many did not. To avoid this inequality of conditions for results which would have to be massed, the subjects in the later experiments were told to 'keep thinking' of a certain definite point.

The following table contains results from 64 subjects tested with the Titchener apparatus. Suggestion may or may not have been effective if a sound is both given and localized in the quadrant or hemisphere or at the point suggested. Such cases are marked with the sign ?. When the suggestion and the reality do not agree, suggestion is perhaps effective if the localization is in the direction of the suggestion and certainly ineffective if it is not. The first class of cases is marked + and the second —. In these experiments the points were evenly dis-

tributed between the right and left, front and back, and upper and lower hemispheres, and the same number of points were submitted to each of two opposite suggestions.

From these figures, it is clear that one effect of suggestion is to reduce the total number of right cases. To be sure, a rather larger percentage of cases are correct with right than with wrong suggestion. In general, however, the number of right cases with suggestion falls below the normal 11 per cent.

It is clear, also, that in the directions FB and BF, errors in accordance with suggestion are slightly more numerous than errors in defiance of suggestion. This statement holds true for errors upward and downward. It does not hold for errors to the right and left.

It looks, farther, as if suggestion diminished the normal tendency toward 'backward errors.' Moreover, in the earlier experiments with the Harvard apparatus, 'backward' suggestion seemed actually to produce a certain amount of accuracy. Here, when the arc FU was suggested (in 295 localizations) there were 4 per cent. of forward errors of over 90° in the median plane, whereas when BU was suggested (in 320 localizations) there were no backward errors of over 90°. On the other hand, in defiance of the suggestion BU, there were no errors of over 90°, whereas in defiance of the suggestion FU there were 10 per cent.

With the Titchener apparatus, the points most correctly localized by unpracticed observers, both with and without suggestion, were B, R and L. With the Münsterberg apparatus the maximum of accuracy shifts with suggestion from RB 60 and LB 60 to R, RF 60 and LF 60.

On the basis of figures which cannot be given, it is clear, finally, that suggestion most influences upward and downward errors. A given suggestion is not especially apt to produce corresponding errors, but any suggestion greatly increases the number of errors upward and downward. Errors forward and backward and to the right and left are actually fewer with suggestion than without it. The number of wrong cases with suggestion is swelled by the many upward and downward errors. Without suggestion, there are about as many of one as

TABLE IV.

SHOWING THE EFFECT OF SUGGESTION UPON UNPRACTICED OBSERVERS.

Suggestion.	Cases.	Cases with Correct Suggestion.	Right Cases, %.	Cases with Wrong Suggestion.	Right Cases, %.	Direction of Error- Cases in Percent.													
						FB.							BF.						
						Under 72°.				Over 72°.			Under 72°.				Over 72°.		
						?	+	−	Total.	+	−	Total.	?	+	−	Total.	+	−	Total.
Quadrants FU and BU.	590	177	6	413	8	5	15	11	*31*	8	6	*14*	7	11	4	*22*	3	2	*5*
Hemispheres U and D.	220	88	10	132	9				*21*			*10*				*26*			*12*
Points F and B.	300	30	30	270	7		12	12	*24*	6	6	*12*		14	11	*25*	4	2	*6*
Points R and L.	80			80	15				*20*			*23*				*14*			*1*
Points U and D.	120			120	8				*26*			*29*				*15*			*.8*
No suggestion.	1442	Right Cases, 11%.							*23*			*23*				*29*			*1*

of the other (of errors UD, 32 per cent. under 72° and 2 per cent. over, and of errors DU, 34 per cent. under 72° and 1 per cent. over, in the sets of results represented in Table IV.). With suggestion, upward errors, large and small, are in the lead.

On the whole, the negative effect of suggestion seems more marked than the positive. Suggestion seems to disintegrate the localizing function. It lessens the tendency to a given sort of error and yet reduces the number of right cases. The thought of any special direction, especially if there have been conflicts between suggestion and reality, seems to produce a kind of paralysis, like that of the famous centipede who, when the toad for fun said, 'Pray, which leg comes after which?' then 'lay distracted in a ditch considering how to run.'

Conclusion.

The following conclusions are suggested for future verification:

1. The perception of the direction of a telephone-click is not usually based on the consciousness of timbre, or of intensity or of pitch, or of any kind of place-mark or space-value in the sound itself.

2. Timbre and intensity criteria develop with experience in auditory localization, and seem in a measure to presuppose it.

3. Cutaneous impressions about the ears and head do sometimes serve as factors in the localization-consciousness.

4. Auditory localization is a rough counterpart of cutaneous localization. It proceeds originally by reflex head- and eye-movements which drop with practice.

The immediacy of auditory localization, if a real directness, cannot well be explained except by the lapsing of reflex movements. Moreover, in ordinary life, we are frequently finding ourselves and others turning the head from side to side to localize some faint sound. Apart from such obvious considerations, the following points are noted as collateral evidence for the fourth conclusion.

(1) Suggestion has no very marked effect on strong tendencies in the perception of direction.

(2) On the other hand, it hinders accurate localization, as thought about an automatic muscular coordination hinders the process.

(3) Unpracticed observers tend to localize sounds behind them. This tendency is perhaps explicable on the basis of serviceable reflex movements in response to a noise.

(4) Alleged immediacy in localization is coupled with relative accuracy in the cases carefully examined.

CORRELATIONS AMONG PERCEPTIVE AND ASSOCIATIVE PROCESSES.

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It has been the habit of psychologists to use the words 'memory,' 'attention,' 'delicacy of discrimination,' etc., as if they referred to general mental functions, and the words 'quickness' and 'accuracy' and 'ability,' as if they referred to general mental qualities. But any consideration of the nervous basis of mental life or of the patent facts of human nature suggests that *à priori* it is more rational to look on the mind as a multitude of particular capacities, particular associations and particular acts, all of which may be highly independent of each other. And the studies of Thorndike and Woodworth[1] on the effect of training, of Wissler[2] on the correlation of mental tests and of Norsworthy[3] on the correlation of school abilities, give actual evidence that such is the fact.

The extent to which any one mental trait presupposes any other is in fact in every case a matter to be actually measured. We cannot tell beforehand what the relationships are. The present study gives measurements of such relationships in the case of a number of functions, all of which depend upon quickness and accuracy in associating certain thoughts or acts with certain percepts either directly or indirectly through other ideas which the percepts call up. The associations were in every case such as involved (in the subjects tested) attentive selection of correct and purposive inhibition of incorrect ideas. They were controlled associations. The functions in question were much more alike than were those examined by Wissler. We

[1] PSYCHOLOGICAL REVIEW, Vol. VIII., Nos. 3, 4 and 6 (1901).
[2] PSYCHOLOGICAL REVIEW, Monograph Supplement No. 16 (1901).
[3] *Journal of Pedagogy*, Vol. XIV., No. 1 (1901).

have examined the relationships between functions in an extremely favorable case.

The results reinforce the evidence showing that functions apparently closely similar may really be to a large extent independent specializations. For instance the ability to call up quickly the opposites of 'good,' 'rich,' 'heavy,' etc., is by no means identical with the ability to call up quickly the letters coming before c, k, t, etc., or with the ability to call up quickly the answers to 7 + 4, 11 + 9, 20 + 6, etc. 'Quickness of association' as an ability determining the speed of all one's associations is a myth. Quickness in noticing words containing the two letters r and e does not to any appreciable extent involve quickness in noticing words grossly misspelled, nor does accuracy in the one involve anything like equal accuracy in the other. The 'attention' and 'discrimination' required in the two cases must therefore be different things.

Our results also suggest the possibility of clearly defining the classes of functions which we may expect to find closely related. For in the tests involving quickness and accuracy in purposive associations there is much closer dependence than in the tests involving quickness and accuracy of perception, though to the speculative psychologist the latter would seem to be cases of the same function.

It may be added that both of these conclusions are supported by the results of many other correlations not yet worked out in detail and so not presented here.

The importance to general psychological theory of measures of the relationship of different mental functions is obvious. Where introspective analysis fails to discover the exact structure of a mental process, the study of its relationships with other better known functions may succeed. Moreover the whole question of the influences of heredity, maturity and training will be illuminated by a knowledge of the necessary bonds and interactions of the different mental processes.

The tests were all given by one person; they were given to 160 boys and girls in the eighth grade (the highest) and to 80 in the fifth grade. Care was taken that in all the tests all the pupils

had the same conditions of explanation, incentive, time, etc. The tests were the following:

1. To mark every mispelled word in this passage (time, 180 seconds):

I percieved, about four years ago, a large spiider in one korner of my room, makeing its web; and through the maid frequentely leveled her fatale brom against the lobors of the little anemal, I had the good fortoone then to prevente its distrucsion, and, I may say, it mor than paid me by the intertainement it aforded.

In thre days the weeb was, with encredable diligence, compleeted; nor could I avod thinkeing that the insect seemed to exult in its new abode. It ofen treversd it round, and exsamined the strenth of every part of it, retierd into its whole, and came out very ferquently. The first inemy, however, it had to inconter was another and much larger spidur, which, having no web of its owne, and haveing probibly hexausted all its stock in former labors of this kind, came to invaide the prouperty of its nieghbore.

Soon a terreble encounter ensooed, in which the invader seemed to have the victorie, and the laborius spider was obleeged to take refug in its hole. Upon this I perceived the victer useing every art to draw the enemey from his strongholde. He seemed to go of, but quicklie returned, and, when he found all arts vane, began to dimoilish the new web withoute mercy. This broght on another battle, and contary to my expextations, the laborious spider became conckeror, and fairly killed his antagonist.

Nou in pieceable possession of what was justely its own, it awated three days with the uttmoste impatients, repairing the breeches of its web, and taking no sustenance that I could perceive. Ate last, houever, a large blue fly fell into the snaire, and strugled hard to get lose. The spider gave it leeve to intangle itself as much as possible, but it seemed to be to strong for the cobwebe.

I must own I was grately serprised when I saw the spider imediately sally out, and in lese than a minite wheave a new nett around its capthive, by wich the moshun of its wings was stoped, and, when it was fairely hampered in this maner, it was siezed and druged into the houle.

In this manner it lifed, in a precarious staite, and Natcher seemed to have fited it for such a life, for upon a singl fly it subsested for a weak. I put a waspe into the neat, but the spider sit it free.

2. To mark every word containing r and e in this passage (time, 60 seconds):

Then the carelessly dressed gentleman stepped lightly into Warren's carriage and held out a small card. John vanished behind the bushes and the carriage moved along down the driveway. The audience of passers-by which had been gathering about them melted away in an instant leaving only a poor old lady on the curb. Albert was sadly striding across the field stopping now and then to look back at the village or staring at the bit of paper which he still held tightly grasped in his left hand. He wished to keep his appointment; nevertheless he felt that he ought to go back to Cromley to ascertain in what direction the stranger might be going. The highway lay on his left seeming

for all the world like a dusty ribbon. Before him gleamed the spire of Relton church. His thoughts flew to his former roving in the days when Clayton and he were working for Fleckmer. "What an insatiable greed for power that man had," he thought, "I on the contrary have really no ambition." He laughed bitterly. "Hopelessly inactive I truly am." A partridge flew up ahead of him. He tried to hit it, but merely knocked off some leaves.

3, 4 and 5. To fill out the blanks A, B and C, thirty seconds being given for A and also for B and fifteen seconds for C.

A.	B.	C.
Write as fast as you can beside each of the words in the column a word that means the opposite thing from it.	Write as fast as you can beside each of the words in the column a word that means the opposite thing from it.	Write as quickly as you can beside each letter in the column, the letter that comes *before* it in the alphabet.
stupid	good	f
hard-working	outside	k
strong	friend	s
sane	quick	p
obnoxious	tall	w
foolish	big	l
handsome	loud	e
adroit	white	r
superior	light	d
loquacious	happy	o
rapid	false	v
generous	like	j
straight	rich	n
separate	sick	t
up	glad	h
always	thin	
joy	empty	
high	war	
obscure	many	
proud	above	

6. To do the following examples (120 seconds):

ADDITION EXAMPLES.

17	26	27	72	23
42	51	24	14	47
38	47	83	39	86
91	82	19	81	54
54	63	45	26	36
17	42	38	91	36
26	51	47	82	26
27	24	83	19	45
72	14	39	62	63
23	47	86	54	54

41	53	67	78	86
52	67	86	37	32
86	34	23	96	44
23	78	45	72	36
35	19	67	23	68
45	52	19	45	23
13	86	78	67	72
68	23	67	78	36
77	35	23	37	68
86	67	86	96	39

In the misspelled word test each individual was given five scores: (1) The number of lines covered; (2) the number of words marked that should have been marked; (3) the number of words marked per line; (4) the number of words marked that should not have been marked; (5) a mark for his general efficiency in the test, obtained by finding in 2 and 3 his distance above or below the average for his grade in terms of the average deviation of the grade from that average, adding the two distances together, and adding or subtracting a small amount to the figure thus obtained in the case of those who were much above or below the average in the number of words incorrectly marked. Similar scores were given in the case of the R-E test.

In the hard opposites (A), easy opposites (B), and alphabet tests (C), each individual was given five scores, namely, (1) the point reached in the test, (2) the number done, (3) the number of errors, (4) the percentage of errors to the number done, and (5) a general score for efficiency calculated differently in the three cases. The method of calculation with the hard opposites was to subtract twice the number of errors from the number done, multiply the product by two and subtract the number of omissions; with the easy opposites do the same, save that three times the number of omissions were subtracted; with the alphabet test twice the number of errors were subtracted from the number done, and a correction made in the few cases where there were omissions according to the nature of each case. These scores of efficiency are to some extent arbitrary and are

possibly unfair. They are not of much importance in the discussion.

Each individual was also given a score for the total number done in all three of these tests, a score for the total number of errors, a score for the ratio of total errors to total number done, a score for the total number correct, and a score for total efficiency obtained by adding the efficiency scores in hard opposites and alphabet to one-half the score in easy opposites.

The scores given in the addition test were (1) number done, (2) number of errors, (3) number of errors per example, and (4) a score for general efficiency obtained by subtracting one-half of (2) from (1).

We have thus for each individual a record like this:

GRACE ALEXANDER.

MISSPELLED WORDS.

Number of lines	31
Number of correct words	75
Words per line	2.4
Incorrect words	2
Efficiency	+1.3

RE.

Number of lines	9
Number of correct words	11
Words per line	1.2
Incorrect words	0
Efficiency	—.68

	Hard opp.	Easy opp.	Alphabet.
Number reached	9	6	3
Number done	7	6	3
Number of errors	1	1	0
Percentage of errors	14	17	0
Efficiency	8	8	3

ASSOCIATIVE TESTS COMBINED.

Total number done	16
Total number of errors	2
Total percentage of errors	12.5
Total number correct	14
Total efficiency	15

ADDITION.

Number done	9
Number of errors	2
Percentage of errors	.22
Efficiency	8

The relationship in the case of the individuals tested between the two abilities measured by any two of these tests will be represented by any figure or diagram or statement that shows how far variations in one of the abilities go with similar or opposite variations in the other. We shall use a single figure for that purpose. Its meaning will be clear from the following derivation in one case:

The relationship between the amount done in addition and the total amount done in the three other association tests.

15 students doing the least in addition, namely from 3 to 6, got the following scores in the three other tests: 2 got 10, 3 got 12, 1 got 13, 3 got 14, 1 got 16, 3 got 17, 1 got 19, 1 got 20. They averaged 14.5. If these fifteen who did the least in addition had been the fifteen doing the least in the association tests they would have obtained the following scores: one 6, one 7, two 9, six 10, four 11 and one 12, and would have averaged 9.9. The general average in the total for the three association tests was 17.6. The lowest 15 in addition were thus 3.1 below the average in the total for the three association tests. If correlation had been perfect, *i. e.*, if an individual's rank in one exactly corresponded with his rank in the other, they would have been 7.7 below. The degree of correlation for them was thus 40 per cent.

36 students doing 7 in addition got in the three other tests, etc.
28 " " 8 " " etc.
35 " " 9 " " "
24 " " 10 " " "
18 " " from 11 to 16 in addition, etc.

Data for all these groups similar to those given above for the lowest 15 are presented in Table A.

TABLE A.

RELATION BETWEEN AMOUNT DONE IN ADDITION AND AMOUNT DONE IN THE THREE COMBINED ASSOCIATION TESTS.

Each figure in the table records that that many individuals did in addition the amount *under* which the figure stands and in the three combined association tests the amount *opposite* which the figure stands.

	3	4	5	6	7	8	9	10	11	12	13	14	15	16	
6					1										
7							1								
8															
9					2										
10				2	3		1								
11					1	1	1	1							
12		1		2	1		1				1				
13	1				3	1	2								
14			1	2	3	3	1	2							
15					3	1	3	1	1						
16				1	7	2	6	2	1						
17			2	1	5	5		2		1					
18					2	2	1	3	1						
19				1		3	4	1			1				
20			1		2	4	4	1	2						
21					2	3	2	5	1						
22					1		4	2		1				1	
23							1			1					
24							1	1		2					
25						1				2	1				
26						1	1	1	1						
27							1	1							
28								1							
29															
30															
31						1									
Number of cases.		15		36		63		24			18				Total 156.
Average amount in association tests.		14.5		15		18.1[1]		19.5			20.8				General average 17.6
Average if perfect correlation.		9.9		14		17.8		21.3			25.5				
Actual deviations from general average.		−3.1		−2.6		+.5[1]		+1.9			+3.2				
Deviations of perfect correlation.		−7.7		−3.6		+.2		+3.7			+7.9				
Percentages of correlation.		40		72		—[1]		51			41				Average correlation 51%.

[1] These cases so near the average should be omitted in computing the general relationship, since they give a different result if grouped differently.

The average of the percentages of correlation for the different groups, gives a sufficiently though not the most accurate single figure to express the closeness of relationship between the two functions. Perfect correlation would be represented, of course, by 100%, utter independence by 0%, and antagonism in the two functions by a figure with a negative sign.

The following relationship was worked out by the method just described. When there are two figures for the relationship the upper one refers to the relationship found in the 8th grade pupils, the lower one to that found in the 5th grade pupils. Where only one figure is given, it refers to the 8th grade pupils alone.

	Per Cent. of Correlation.
Misspelled words marked (2) and R. E. words marked (2). Quickness (and to some extent accuracy) of Perception	0 0
Misspelled words per line (3) and R. E. words per line (3). Accuracy of Perception	16 25
Misspelled words (efficiency (5)) and R. E. words (efficiency (5))	None or slight.
Hard opposites (number done (2)) and easy opposites (number done (2)). Quickness of association	50 50
Hard opposites (number done) and alphabet (number done). Quickness of association	22 19
Alphabet (number done) and easy opposites (number done). Quickness of association 116 cases from another school	40 50 41
Hard opposites (percentage of errors) and easy opposites (percentage of errors). Accuracy of association	20
Hard opposites (percentage of errors) and alphabet (percentage of errors). Accuracy of association	10
Alphabet (percentage of errors) and easy opposites (percentage of errors). Accuracy of association	40
Hard opposites (efficiency) and easy opposites (efficiency)	40
Hard opposites (efficiency) and alphabet (efficiency)	34
Alphabet (efficiency) and easy opposites (efficiency)	60
Association tests (total number done) and addition (number done)	50 20
Association tests (total percentage of errors) with addition (percentage of errors)	None or slight.
Association tests (efficiency) and addition (efficiency)	48
Association tests (total number done) and R. E. (number of words marked)	None or slight.
Association tests (total number done) and R. E. (lines marked)	8 or less.
Association tests (total number done) and misspelled words marked	20
Association tests (efficiency) and R. E. (efficiency)	None or slight.
Association tests (efficiency) and misspelled words (efficiency)	50
Addition (number done) and R. E. (number of words marked)	None or slight.
Addition (number done) and misspelled words marked	20
Addition (efficiency) and R. E. (efficiency)	None or slight.
Addition (efficiency) and misspelled words (efficiency)	50

DISCUSSION AND REPORTS.

IMITATION.

May I offer one or two remarks on Professor Baldwin's references to my views in your issue of January, 1902, which, owing to my absence from England, did not reach me until March? I am persuaded that when we differ on psychology Professor Baldwin is almost certain to be right. And yet I cannot feel that my difficulties are met by what he has said; and I should like to try and make my point clear.

First, I will restate my difficulty in general terms. The question is, as I understand it, which of two suggested principles helps us most in explaining that transference and operation of ideas by which men are social. Professor Baldwin says 'imitation'; I say 'logic.' Neither of us, I believe, holds reflective consciousness to be necessary to the operation of the principle. For him, this operation *is* imitative process, and results in producing, transferring or realizing resemblances of copies—on the whole, in repetition. For me, it *is* logical process, and results in producing, transferring or realizing systems of connected differences—on the whole, in organization. His principle works by similarity; mine by identity in difference. My difficulty is that I cannot see how development into a group of interrelated elements—something organic, a design, or pattern, or situation—can be got by imitative process proper. I admit and maintain that every mental process tends to such development; but this seems to me to show, not that pure imitation can organize, but that mental process cannot be purely imitative.

Is it a verbal question? Has imitation been construed to cover logical process? If so, that explains our difference. Or (a somewhat different case), is 'imitation,' taken in a certain technical sense and with certain additions, proposed as the scientific or psychological *explanation* of logical development? I suspect something like this to be meant. But if so, I think this is more than a verbal question, so long as imitation retains anything of its primary meaning, viz., the production of resemblances. My view is that imitation is a case of logical process, in which differentiation of the copy, always present, is disregarded *ad hoc*. To build up logical process out of imitation seems

to me, therefore, to be a topsy-turvy arrangement. It seems to me to found the theory of a great subject on an extreme subcase, and on only one half of that.

1. To begin with, Professor Baldwin seems to urge that imitation is scientifically useful and identity in difference is not—to make our difference, in short, one of science versus philosophy. Now, I believe I know what is meant by a working hypothesis; but I can see no advantage in its being further wrong than it need be. It is obvious (pp. 57–58) that 'identity in difference' solves no question by being merely mentioned. But the same, surely, must be true of 'imitation.' If, however, either is to be applied in analysis, then I think that the 'sociologist, statistician, etc., etc.,' would gain, by using the former, a higher degree of delicacy in their solutions, and perhaps the avoidance of serious blunders—such as I have pointed out in LeBon's confusion of a crowd with a state, and Durkheim's separation of penal and civil law. Space forbids me to analyze the instances which Professor Baldwin adduces—the cotton-gin and the lynching party;—but if it is implied that a lynching party is pure 'follow-my-leader,' or the cotton-gin pure invention, I suspect that the analysis would bear revising in the light of a less crude principle. Even in biology it is conceivable that another point of view might be helpful in discussing the degree of generality of inherited characters or tendencies. Might not, for instance, that which appeared as drink in the father appear as poetry in the son, simply by reaction to different surroundings in life, and without prenatal 'variations'? If so, would not an error of method be committed by looking for a special cause of variation in such an instance? Such considerations are not foreign, surely, to post-Darwinian controversy,[1] and it was Darwin's own care and skill in defining the general nature of certain inherited tendencies which first impressed me with the need for caution at this point ('Origin of Species,' 6th ed., p. 209).

I have purposely, for shortness' sake, given myself away *prima facie*, by saying that my principle is 'logic.' It may be retorted at once, 'Logic cannot be a principle in psychology.' But this would suit me; for I only ask to know where we are; that is, what consequences will be drawn from this assertion. Logic is the shortest expression I can think of for the operation of ideas in organizing wholes of thought. If, in psychology, you cannot call it logic, still you must deal somehow with this operation of ideas, and I want to know how it is dealt with in the theory before us. Is it explained by

[1] *E. g.*, Weismann, Biological Memoirs, Clarendon Press, 1889, p. 393.

imitation alone, or by imitation plus something else—selection, perhaps—or is it, as I suspect, to a great extent omitted?

2. The imitation theory derives an undue advantage from the application of such terms as 'general' and 'collective' to the 'general will.' This favors a loose notion that the general self consists of the common elements, roughly called resemblances, among a number of wills. But in principle the will is always a system, and the contribution of each unit is individual, related organically, and not as resemblance, not as a common element, to that of the other individuals. This necessity, as I understand, Professor Baldwin accepts and maintains in his doctrine of the 'thought situation' and publicity. But with the recognition of this necessity, imitation ceases to be a plausible explanation, except in a subordinate sense which I will state directly. *Prima facie* imitation can never give the thought of a situation. A thought situation means an idea of relation between things and persons variously connected, but so differing as to form a coöperative whole. I do not see how imitation as such can ever produce an idea of the definite difference and correlation, within a situation or pattern, of the imitator and the original. For this it is necessary that a principle or content, which may be suggested no doubt by any object of imitation, should take on a different but corresponding shape in the coöperating units; and that the different shapes should be held together, and should operate in conjunction, as elements of the whole content which now includes these differences. The value of pure imitation must depend, so far as I understand, on the complexity of the individual's nature. If the situation involves a child and a teacher, then, say, for a third person dealing with it, it will be a help to have reinforced, as if by resonators, the child nature and the teacher nature within himself, through an experience which may fairly be called imitation. But the grasp of the relation between the persons involved belongs to the mental process as a whole, and not to its imitative side.

I do not see the initial difficulty of obtaining such a grasp, which is the condition of a general will or self. It is implied in mental process from the beginning, through the principle that contents operate as universals, assuming differences in accordance with the details through which they are reproduced. As regards the participation of different intelligent individuals in one such grasp, the true principle seems to me to be that an idea normally operates throughout several intelligences just as within one. It is obstruction and lack of communication that is, in principle, abnormal. Communication depends ultimately on logic; language and concerted action are incidental

cases of it. I mean that in all cases the ultimate nerve of intercourse is that, according to a nature common to all concerned, one thing follows from another—it may be act, speech, thought or feeling. The thing so following, whether in myself or in another, I equally recognize as the continuation and completion of my thought. Of course I constantly fail to understand, but in as far as I do understand, whether through common action, or language, or perception, or feeling, that is what my understanding means. I want, perhaps, food, rest, tendance in sickness, a railway ticket, a laugh, sympathy, a fight. I get what I want, it may be by words, signs, looks, or by simply presenting myself. This is all incidental. The nerve of the whole process is that given the data, including their own resources, other minds bring out, in a form prescribed by their powers, the conclusion at which mine is aiming. The point is, that it is a *conclusion*—that is, something new, and different from the premises, and consisting for the other minds not in a copy of my state or action, but in another state or action definitely relevant to it in consequence of a common principle which connects the two. Here we have the essentials of the thought situation; and its development into explicit unity is only a matter of the degree of reflective consciousness.

3. Has Professor Baldwin at last (p. 67) done justice to logic by his doctrine of selective thinking? It appears to be intended to supplement the doctrine of imitation by showing how, in applying a copy to new conditions, variations suitable to those conditions arise. It should be then, in fact, a theory of the logical process as above stated. And such a theory, in psychological terms, would be welcome.

The question of principle seems to be whether selective thinking means selection of workable combinations out of indeterminate variations, by trial and error, or construction of a determinate adapted variation by logical process. The prerogative of reason surely is to ascertain, without trial and error, that is, without actual working upon things, what adaptation is workable in a given situation. I will take calculation as a very simple instance. When I have calculated how many trees are needed to plant an acre of ground at a given distance apart, the adaptation is fully determinate. There is no room for variation or for selection. "Selection of actions which 'work' in a given situation" would most naturally mean the sort of thing which we are told a dog does in trying to open a gate, viz., selection by trial and error. If it means this, it omits the essence of calculation and inference, in short, of reasoning. If it is to cover the facts of reasoning, then the term 'selection' must be slurred, and the meaning of

the term 'work' or 'workable' extended to cover all the ways in which inference can be confirmed. But if so, we are making logic its own explanation. 'Actions which work,' 'workable combinations,' will include the results of theories which 'work,' that is, which agree with experience in any form, practical or cognitive.

Thus, when we hear that 'thoughts are the counterparts of former adapted actions' we may interpret this in two ways. It may mean that a thought is the memory or at least the effect of a lucky hit, such as a dog makes in lifting the latch of a gate; in this case there was no principle of adaptation in the action, and the thought, as dependent on such a process, has none either. Each step depends on trial and error, and advance depends on a succession of lucky hits. Or it may mean that a thought is a phase of adapted principle, carrying on in a new situation the idea which gave a successful conclusion before. But in this case thought depends on previous thought of the same kind; the variation becomes predetermined; selection vanishes, and with it the prerogative of actual working on things as a test. For all thought whatever is a principle at work, in the sense in which calculated action is so; and whether the conditions to be met are materially present, or only present to knowledge, can involve no difference of any kind to the nerve of the process. It is only in trial and error that practice on material things has a prerogative over inference. When we come to calculation and reasoning the special import of material action disappears, and it becomes one among many forms of cognition, all of which are tested by 'working' in the wider sense. Take the instance of building a house (I quote the passage):[1] "I should say that the plan of the whole is made up of parts each of which is taken imitatively from other houses or plans of houses, or selected out by the owner himself from alternative variations of thought, by the process of getting new workable combinations, which is indicated above."

What is meant here by 'getting new workable combinations'? It may mean (i) that you try putting together parts borrowed from other examples—to be consistent, you should try them in actual bricks and mortar—and when you find a combination that does not tumble down, you accept it, like a child building with wooden bricks, or perhaps a workman by rule of thumb. This would be true selection, of variations taken imitatively, according to what is found workable in material action.

But the moment you start even to draw a plan, or to calculate dimensions, not to speak of reasoning to a general effect, something

[1] PSYCH. REVIEW, IX., p. 67.

quite different begins to take place.[1] (ii) A whole comes into being, whose nature enters into and affects the nature of every part, and by the necessity of the case the logical unity, which in this case is called design, asserts itself throughout. It is true that there must be suggestions; a modern house could not be built if no house had ever been built before. It is, I think, not true that the suggestions can be taken imitatively, though design of a low type may attempt something of the kind. Every one must be recast as it is taken up in the whole. Now the point is that in this case the expression 'getting new workable combinations' has lost its distinctive meaning as an explanation of thinking by something else, viz., choice in action. 'Workable' no longer means what you find to work by actual trial, but merely what is workable in a logical sense, *i. e.*, what is self-consistent or even what is satisfactory in the world of architectural experience. Selective thinking, in short, is an improper phrase, because thinking as such is selective and more than selective, being selective, so to speak, by construction of determinate adapted variations. Professor Baldwin has a leaning towards determinate variations.[2] I believe that this is the crux of the whole matter, and that if the origin of determinate variations is fairly considered the idea of selective thinking must go. I do not believe that an explanation of logical process can be built up on imitation plus selection, and the facts seem to me to be in the main omitted by the imitation theory.

4. I will not call Professor Baldwin an associationist; for no one should be called by a party name which he disclaims. But I will explain what I meant by saying that I was not sure whether to call him so. I simply meant that he seems to me to deny or to neglect logical process. Thought, as I understand him, is essentially reproduction of resemblances plus acceptance of variations. In its own nature, thought has no synthetic unity, but only repetition. All unity is derivative from the test of action; it is motor unity, synergy, unity of action. Thought repeats anything that is offered it; it is action that solidifies in a system that which is coherent on actual trial, and shaves away the superfluous or discordant. Now I see the vraisemblance of this. For good logical reasons material action is an incessant induction. But the crux of our question is, *why* is material action an excellent test of thoughts? My answer is, because in assignable respects it is a very strong case of thinking with verification. But if

[1] Cf. Stout, 'Anal. Psychology,' II., p. 60. I do not know whether, as Professor Baldwin seems to hint (p. 68), Mr. Stout has abandoned the essence of the view here expressed.

[2] 'Social and Ethical Interpretations,' p. 96.

you make thought borrow its unity and consistency, having none in itself, from what is found to succeed in action, then I no longer see why we should respect these qualities or treat them as guides to truth or reality. Action no longer reveals a principle that pervades experience. On the contrary, experience is erected on *de facto* successes in action, with the origin of which no nexus or principle in experience has anything to do.

This view, then, seemed to me associationist because it made thought capable only of joining or reproducing the given, and not of carrying out a principle through determinate variations, adjusted to presented details, into an organized system. If, to do this, thought rests on action which is not thought, then action in doing it rests on nothing.

I believe I see the strength of Professor Baldwin's view. It is that in thinking everything comes from somewhere. We make nothing 'out of whole cloth.' Therefore, it is natural to speak of imitation and selection. But if we can make nothing new, on the other hand we can take nothing as it stands. We take always for a reason and in carrying out a connection; and this connection modifies what is taken. As taken, therefore, the elements of our thoughts, though not new, are original. They rest on a reason, not on mere trial.

These are the difficulties which I wished to have cleared up. I am quite ready to believe that I have missed Professor Baldwin's intention. But I am very strongly convinced that his view needs at least restating.

BERNARD BOSANQUET.

MR. SUMNER'S REVIEW OF THE PIPER REPORT.

It is probable that most of those who read Mr. Sumner's remarks on my Report of the Piper case will not read the Report itself. I therefore ought to correct some of his misapprehensions and misrepresentations. I shall not enter into any defence of the hypothesis entertained in that Report. This is not called for in the PSYCHOLOGICAL REVIEW, which is devoted to more important matters.

But first let me express my perfect agreement with the Editor, Professor Cattell, in the view that 'the phenomena are not suited to scientific investigation until the possibility of fraud has been excluded.' This statement is so true that I think it applies to all experimental psychology, and perhaps the results in some cases would be much more respected if we had been shown how frauds had been excluded from the work. But I am glad to see the maxim recognized even at

this late date, though I would not be surprised to discover that even psychical researchers had felt some fear that it would some day be suggested as important. They will certainly congratulate all scientific men on the discovery of it and for the solicitude that would keep us from going astray. But I must emphatically differ with the Editor on the matter of placing psychical research publications in the hands of sympathizers with it, as this might compromise the reputation of the REVIEW and encourage the favorable notice of ideas that might better be stamped out. I rather suspect that it would conduce more decidedly to scientific truth to put such matters into the hands of men who are able to teach us something on such problems. I do not read one per cent. of the reviews of books for the very reason that the maxim here announced by the Editor has so generally been followed. Moreover, if psychical research cannot stand the animadversions of its intelligent opponents it is not worth much.

Let me now enumerate the misapprehensions and misrepresentations of my critic, Mr. Sumner.

1. His very first allusion to my remarks on *suggestion* as an explanation at least apparently implies that I admitted it as a *fact*, when I was very careful to say that it was 'a conceivable explanation in a few isolated instances.' This he quoted, but does not represent rightly.

2. Mr. Sumner apparently does not note, he certainly does not call any attention to my remark that suggestion is worthless unless it has some applicability to the whole of the record, and particularly to *incidents*, in not one case of which has he mentioned an instance of suggestion. Whatever theory is adopted, even that of fraud, must have some unity and simplicity and not be a system of 'cycles and epicycles' of fishing, guessing, suggestion, telepathy, clairvoyance, wireless telegraphy analogies, secondary personality in a rather supernormal form, and what not. After having admitted the supernormal in the case Mr. Sumner ought to have seen that suggestion has no other place in the matter than a possibility, in so far as evidence is concerned.

3. My question about my mother does *not* 'obviously imply' her decease, as Mr. Sumner says it does, unless you assume that such a question can have but one motive. I tried to trick the 'communicator' with the same question about my brother George, who is still living, and I finally and spontaneously got the right answer with an interesting statement about my question. Mr. Sumner's objection here ought to have been that it was a case of *guessing*, not one of

suggestion. Besides he might have noted what I said (pp. 146–7) about the incidents relative to my mother.

4. In regard to the name McClellan, which has a more plausible claim to explanation by suggestion, I may say that I once wrote out a note indicating precisely the circumstance that Mr. Sumner mentions, but when I observed that the mistake in the name on one day was committed again twice the next day (pp. 427 and 429) after I had pronounced it the previous day and before it was spontaneously corrected in a dramatic manner, I saw that it was not properly a case of suggestion at all, and so decided to omit the note. Besides, why did not Mr. Sumner try to indicate how 'suggestion' worked in regard to the name Cooper, also uttered directly in connection with the name McClellan?

5. A much more serious error of Mr. Sumner is the fact that he reverses the chronological order of the facts in the record apparently in order to make out a plausible case of suggestion. He refers to this mention of the name McClellan before it was correctly given after he had indicated that it had been given with approximate correctness. This gives the appearance of the 'suggestion' which the record does not support.

6. In regard to the name and relationship of my father and Mr. Sumner's remarks thereon, several things should be said. (*a*) Mr. Sumner does not quote *all* the 'recorded facts.' (*b*) He omits to mention what was said by Mrs. Piper as she came out of the trace. (*c*) He omits the most important part of what I said in my note about the mispronunciation of the name after I had uttered it. (*d*) What kind of 'suggestion' is it to have spoken the name correctly *twice before I uttered it at all?* (*e*) What kind of 'suggestion' is it to have pronounced it wrongly after I had pronounced it correctly?

7. In the case of the name of my sister Hettie, Mr. Sumner does not tell the reader what I myself said of it (p. 68), but speaks of it in the list as if I had not considered it amenable to the suspicion of suggestion. I actually stated that I deliberately helped in the getting of that name. I did this for reasons apparent in the experiments on the identification of personality where I refused to permit the sending of the communicator's name. Besides, the case is more probably amenable to the theory of guessing than that of suggestion.

8. The only instance besides the last in any way liable to the charge that it is explicable by suggestion is not mentioned by Mr. Sumner at all, though I intimated the fact in my note. It is the name Clarke for my uncle. Why not tell the reader the facts of the two cases?

9. Mr. Sumner speaks of the list, using my phrase, 'isolated instances,' in quotation marks, as if there were many of them in the record. All but one in his list (and this may not be an exception) are not instances of suggestion, but if objection be taken to them they should be called guessing, a much simpler theory. Then near the close of the review he says: "The information given is certainly (sic) derived in great part from suggestions received from the sitter," etc., *ipse dixit*. But not one iota of evidence for so positive and general a statement. He does not show how a *single synthetic incident* is due to the suggestion he intimates.

10. Mr. Sumner's solicitous fear that I have 'thrown scientific caution to the winds' he illustrates by quoting the record regarding incidents connected with my father's death, and then insinuates that they have no pertinence for a death from 'something like cancer of the larynx.' Possibly he did not read, but he certainly does not tell the reader a word of my very full notes (pp. 328–9, 356–7) showing the great pertinence of this very passage in all its confusion. Besides, if he had referred to what I had said about such confusion (pp. 214–238, 249, 280–285; especially 284–5, and 643–649) he would have understood, supposing that he knows half as much about secondary personality as he would have us believe, why I considered such incidents as 'remarkable.' But it seems to have been important to omit all reference to facts in order to give force to innuendos. I fully understand and respect perplexities regarding passages of this kind, but when criticism is indulged in I am entitled to a full and correct statement of my position regarding them and not a garbled account.

11. In the allusion to my reconstruction of a 'message' regarding a trip with my father Mr. Sumner does not tell the reader what I had said about this very reconstruction *after* I had made it, and that I had condemned it in as strong terms as he does, though I intimated that it was a possible view (p. 409). I spoke in strong terms of such a procedure. Mr. Sumner refers only to certain statements of an earlier date about the 'message' (p. 371). Neither does he tell the reader what I had said about the retention of earlier notes bearing upon just such incidents (pp. 19, 301–2).

12. Mr. Sumner commits the same sin in reference to the name *Bartlett*. He does not tell the reader that I spoke of the possible interpretation in much stronger terms than he had done (p. 404). Besides, when he says that "no such person (as Bartlett) was known by the 'spirit' in life," Mr. Sumner supplies the statement from his own imagination. There is no such statement of fact in the record.

My statement of the facts was a very different one. What would Mr. Sumner say if I remarked that *Bonner* was the name of the principal of the high school to which reference was made in the 'communications,' and who was probably the man with whom my father had talked about my brother?

13. I cannot understand why Mr. Sumner should adopt my sentiments about the last two matters without acknowledging them and then consider that he had presented objections to the case. This is unusual criticism. When his opinion is the same as mine I cannot see, outside of Hegelianism, how it can be opposed to mine.

14. I would like to have it explained why it is 'credulity' to believe in spirits, which are nothing more than the extension in time of a known cause, a human consciousness, and why it is not 'credulity,' on the same evidence, to believe in the supernormal, telepathy, etc., which is wholly an *unknown* principle of explanation. Why is it so scientific to believe in any inexplicable miracles whatever instead of perfectly explicable spirits which act according to known mental laws?

15. In saying that I have probably 'underestimated the histrionic possibilities of secondary personality,' Mr. Sumner neglects to tell the reader what I said about this very thing in referring to the trance personalities (pp. 153–4, 264, 265–6, 292). I distinctly asserted that we had to assume the very utmost in this regard and that the trance personalities must be assumed to be the secondary personality of Mrs. Piper from the standpoint of personal identity which was the criterion in my Report.

16. Mr. Sumner misapprehends my observation that the 'difficulties and objections' to the spiritistic hypothesis are to be found outside my record when he quotes from it the instances which he treats as objections. He ought to have seen from my discussion (pp. 214–238) that such instances as he refers to are treated as *favorable* to the hypothesis and not as opposed to it. They are phenomena that require to be made *intelligible*, but they are not 'difficulties and objections.' It is our knowledge of secondary personality and possibly of the supernormal independently of the Piper case that affords the clue to objections.

17. Mr. Sumner thinks that I have 'ignored the formidable array of difficulties presented by Mrs. Sidgwick.' I reply that I did not ignore them. I considered them in my general treatment of the subject. I had read Mrs. Sidgwick's paper, and have also seen the original records in nearly all the cases mentioned by my critic. But scientific procedure did not allow me to consider questions involved in records

not yet published, and when these are read it will be found that they do not present such 'formidable difficulties' as are here imagined. It is strange that Mr. Sumner should praise me for referring the reader to my detailed record where he could find how mistaken I was in my discussion, and then quote Mrs. Sidgwick's opinions and discussion without waiting to see the original and detailed records. He might have applied his own advice in the last sentence of his review, namely, 'await more data.'

18. In regard to Dr. Hodgson's sittings and the promise of my 'father' to give tests Mr. Sumner says: 'In spite of these promises, the subsequent statements are as confused and as far from the truth as before.' Not all of them. Mr. Sumner neglects to tell the reader that 70 per cent. of them were entirely correct, and 16 per cent. partly correct. His remark applies only to 14 per cent. (p. 119). It might have been well to inform the reader also that I had discussed the questions involved in this 14 per cent. (pp. 214–238).

19. Referring to expressions that were not characteristic of 'communicators,' and using the fact as an objection to the spiritistic hypothesis, Mr. Sumner neither tells the reader that I had specially remarked their non-characteristic nature nor hints that I had specially referred to them as objections to the telepathy which he is willing to accept on the characteristic facts. He simply refers to them in a way which implies that he had picked them out in his own reading, and tries to distort them into an argument against my theory without telling the reader that I had used them in favor of it because against telepathy.

20. In regard to the 'appearance' of various historical notabilities, such as Mrs. Siddons, Bach, Homer, Ulysses, etc., the statements about Phinuit, and Adam Bede, and about the 'spirit' who was alive and well, most of which were taken from Mrs. Sidgwick's paper, I would only repeat Mr. Sumner's maxim, 'Await more data.' As said above I have seen the original records and they do not present any 'insuperable objections' to one who understands secondary personality, as Mr. Sumner wishes us to believe he understands it, and the possibility that this is the mental condition of the 'communicator.' I had so emphasized this point (pp. 249, 284–5, and 643–649), and its bearing upon such cases ought to have been seen and mentioned as sufficient indication of what the theory was and upon what it depended for its full character.

21. The contradictions charged to Phinuit and implied of other personalities do not affect the question of their *existence*, but only their

reliability for telling the truth as to their identity. If contradiction disputed the existence of any being I fear that we should have no reason to believe in the present existence of the human race.

22. Mr. Sumner says: "The 'father' in these communications frequently mentions an 'Annie,' seeming to refer to this stepmother whose name was Maggie." This is not correct. It was not 'Annie' that was frequently mentioned with the apparent reference mentioned. I was careful to say that it was first 'Mannie' and then became 'Nannie' (pp. 47, 342, and 365–6). Either the typesetter or Mr. Sumner made, in this name 'Annie,' and Mr. Sumner did not correct it, but repeated it when the proofs were read, the very same mistake that was made in the 'communications' of my record and which I commented on in Note 95 (p. 536) and the obverse of which was committed at another time and commented on in an interpretation (pp. 231–235, and 536). If living normal persons make this kind of mistake regarding the identity of any one, is it surprising that discarnate spirits might confuse their own identity and that of others when it is supposed that something like secondary personality is their mental condition while communicating? Whether this hypothesis is true or not, it is a part of the theory defended on the evidence of the record (pp. 643–649), and covers just such facts as Mr. Sumner mentions as 'difficulties and objections,' and the theory should be criticised accordingly.

23. In a footnote regarding my statement about the character of Imperator Mr. Sumner fails to specify what the inference is that the reader 'might easily' make, and so innocent was I of an inference, of which even Mr. Sumner is apparently doubtful, that it was a long time before I could even conjecture what he had imagined. I deny that my statement justifies any inference whatever, easy or difficult, regarding the *reality* of Imperator. We speak of the character and purposes of Caliban, of Faust, of Mephistopheles, etc., without being suspected of dealing with realities. From what I had said over and over again about the personality the inference that 'might easily' have been made is that I was making a statement which was intended to be true on any theory whatsoever, even secondary personality and both conscious and unconscious fraud, and not insinuate an interpretation of which he is apparently doubtful himself. Besides, it would have done no harm to have told the reader that I had said in the very next sentence that the main evidence for my statement was based on evidence not yet published, and then have 'awaited more data.'

24. Mr. Sumner calls my references to the 'serious difficulties in

the way of communication' and to the 'mental condition of the communicator' assumptions, as if to insinuate that they were without evidence. Now 'assumption' is a very equivocal term, doing duty for every imaginable degree of belief between pure imagination and an intuitive certitude. 'Necessary assumption' is an expression we often hear for Kant's 'categories' whose validity seems very little to be questioned. I might show that 'serious difficulties in the way of communication' could be a legitimate *a priori* assumption, but I did not indulge in that sort of science. My statements were based on empirical facts and are true on any theory whatsoever, except conscious fraud. The evidence on which I relied is precisely the same as that which Mr. Sumner accepts in favor of the supernormal and is wholly empirical. Does Mr. Sumner mean to imply that there are no difficulties or mental confusion incident to telepathy and secondary personality? If men would only study the psychology of the record as they are made by their superiors to study Wundt or Kant they might discover that a little knowledge of the subject would go as far as contempt in understanding the problem.

I repeat that I am not defending the theory maintained in my report. That seems to have no importance compared with the problems of reaction time, fatigue, space perception, etc. Nobody, of course, is interested in any questions but these. But I do wish to see a perfectly fair and truthful account of the position which I had taken in my report.

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PSYCHOLOGICAL LITERATURE.

Studies in Auditory and Visual Space Perception. ARTHUR HENRY PIERCE. New York, Longmans, Green and Company. 1901. Pp. vi + 361.

The essays published under this title constitute the first fruits of the Kellogg University Fellowship of Amherst College. The college will be fortunate if the admirable standard established by the present volume is maintained in subsequent publications.

The central problem of the book, which furnishes the principle of unity for a number of otherwise independent inquiries, is the investigation of the actual rôle played by experience in the upbuilding of our visual and auditory space-world. Professor Pierce states frankly that his standpoint is that of a flexible nativism. One may surmise that this view was firmly founded before the author entered upon his experimental work. There is not, however, the slightest evidence that his bias as regards a general space theory has invalidated in any particular the catholicity of his experimental program, or the critical acumen with which he reports upon his facts.

The seven essays dealing with visual space are all devoted to optical illusions. The articulation of this portion of the book is somewhat looser than that of the first half, which consists of an extremely systematic and all but exhaustive study of auditory spatial experiences. So far as the reviewer is aware, there is at present nothing extant which will compare in breadth, lucidity and completeness with this treatment of aural space. An excellent historical résumé of previous theories and observations introduces the report of the author's experiments. These are too extended and intricate to admit of detailed description. They involve, however, tests upon ordinary localization with one, two and three sounds respectively under the most various conditions; tests upon the localizing of sounds in the median plane, upon the effects of the outer ear, upon the idiosyncrasies of intracranial localization, upon monaural localization and upon general auditory orientation, including the probable part played by the eyes and by the semicircular canals respectively. The perception of distance as distinguished from direction is accorded an interesting series of experiments, in which the importance of auditory quality seems quite clearly

made out. The positive doctrine, which issues from all this material, as regards the fundamental principle in ordinary localization of sound, involves the rejection of the several forms of 'semicircular canal theory,' together with the theories involving tactual sensations in the shell and drum of the ear, and the adoption of a view making the distribution of sound intensities to the two ears the matter of primal significance, supplemented at times by the changes in acoustic quality which sounds undergo by reason of the modifying influences of the pinnæ and the bones of the head. The concluding section of this part of the volume reverts to matters opened up in the introduction and presents considerations which, in the author's opinion, constitute a definite support for his nativistic position.

Dr. Pierce regards auditory sensations as unquestionably spatial in intrinsic nature, but he distinguishes sharply between a positional space and a space of extensity, and it is the former variety of spatiality which he believes to be the undeniable possession of sound. The voluminousness of sounds he regards as a characteristic which can neither be proved nor disproved conclusively. The balance of evidence he thinks, however, favors the genuineness of this attribute. As this latter doctrine is somewhat tentatively maintained, we may properly disregard it and dwell a moment upon the claims of the author's positional auditory space.

Let it be said at once, that Dr. Pierce frankly concedes the practical primacy of a space dominated by visual-tactual-motor elements. The localization of sound is consequently a process largely dependent upon the manipulation of these factors. The auditory positional space for which he contends is not, therefore, a thing capable under ordinary conditions of introspective isolation. The one piece of unequivocal (?) empirical evidence for the intrinsic independence of auditory space which the author adduces, is the fact of intracranial localization of sound. As no visual, tactual or motor elements can apparently contribute to this experience, Dr. Pierce concludes that it can only be made intelligible on the basis of localization in purely auditory terms.

It is difficult to follow the author in his assurance as to the conclusive nature of this evidence for an aural space. Of course no question can be raised as to the relatively unimportant part played in these intracranial localizations by vision and the usual forms of tactual-motor experience. For argument's sake one may, indeed, grant that these last-named elements play no part whatever. But surely the facts cited by Dr. Pierce himself regarding the extremely variable nature of these localizations would suggest that some widely variable factors were

responsible for the result and not the mere vagaries of an auditory space sensation, which surely should not be so hopelessly capricious. Now the precise variable desiderated is at hand, if we assume that these localizations are, like so many other extracranial instances, cases in which the position accorded the sound depends upon concomitant sensations from other senses, such as vision. For the mechanical stimulation of the bones of the skull and the sensitive tissues within the cranial cavity is an almost inevitable incident of the conditions producing these intracranial localizations. Whenever such mechanical stimulation of these structures does chance to occur, it may easily furnish a point of reference for the sound to attach itself to. The ease with which sounds located in the median plane may be made to attach themselves to quite erroneous positions tends to confirm this view. The varying density of the bones, the varying modes of conjunction, the differing sizes and shapes of the cavities of the bones, all these and other similar factors are possible sources of the fluctuating nature of these localizations experienced by different individuals.

Dr. Pierce is naturally aware of all this and the disagreement reduces itself largely to a question of the weight to be accorded the several possibilities in the case. Dr. Pierce regards our suggestion as practically irrelevant. Introspection does not clearly reveal anything beyond the auditory sensation. According to well-established psychological doctrine, this could hardly be expected to be otherwise. The two kinds of sensation, if there are two, are always conjoined and cannot, therefore, be readily analyzed. That the sound dominates in the supposititious complex is no more to be marveled at than is the corresponding dominance of taste in certain taste-smell fusions. The interpretation we propose is by no means capable of conclusive demonstration from any adducible evidence. But it is surely a tenable hypothesis and one which is available for those who cannot accede to the doctrine of an auditory space.

Several of the essays on visual illusions have already appeared in substance in the pages of the PSYCHOLOGICAL REVIEW and *Science*. Four of the papers are concerned with illusions of movement and three with illusions of linear direction and contour. Dr. Pierce has been extremely clever in working out experimental conditions for examining the cases with which he deals and many unsupported speculations of other writers are given a *coup de grâce* by his observations. It is of course impossible to bring all his explanatory theorizing under any single rubric. But the position which is most important and most characteristic is found in the rôle assigned to purely

retinal conditions as opposed, for instance, to muscle sensations and the other stock machinery by which visual illusions are commonly explained.

As representatives of this tendency may be mentioned the author's conclusive demonstration that the illusion of the 'kindergarten patterns' is due solely to irradiation, and his persuasive argument that the illusory movements of the columns in the Zöllner diagram are due to a natural interpretation — following our common experiences — of actual movements upon the retina.

The condition of confusion and disagreement among psychologists concerning the whole range of visual illusions is a somewhat scandalous thing at best, and such searching experimental investigations as these of Dr. Pierce are on that account, to mention no others, sincerely welcome. Moreover, the line along which he has chiefly sought his explanations is altogether in accord with the mass of theory in the cognate field of color sensation. It certainly stands for concreteness and simplicity and to the reviewer's mind at least much of it is convincing.

There is necessarily considerable critical comment throughout many of the papers and Heymans, Wundt, Judd, Thiéry and Helmholtz come in for perhaps the lion's share. Two papers, one on the illusory dust-drift seen after the fixation of narrow stripes and the other on two illusions of double movement, contain reports of phenomena which the author appears to be the first to have observed in these forms at least. The first illusion involves an after-image process under the influence of impulses to movement; the second depends upon conditions similar to those of the Münsterberg-Jastrow phenomenon. The explanations advanced are somewhat too tentative and vague for profitable discussion. They are, however, probably all that can safely be said at the present time, a fact that reflects not on Dr. Pierce primarily, but on the prevalent ignorance concerning the causes of optical after-effects from stimulations of movement. The facts observed are extremely interesting and must be reckoned with by any theory which attempts a systematic account of visual processes.

No courageous student of visual illusions can refrain from a tilt at the Poggendorff illusion, whose insidious apparent simplicity has worked havoc with many a trustful theorist. Dr. Pierce is no exception to this well-established tradition. It must, however, be confessed that his careful analyses have enabled him to come off much better than many of his predecessors. He advances seemingly crucial evidence to show that the most important element in the illusion is the tendency

to overestimate the vertical dimensions of the figure. Subordinate to this factor, but exercising distinct and varying influence under various conditions, are to be mentioned the width of the band formed by the parallels, the overestimation of *one* of the acute angles relatively to the other (a fact which seems previously to have escaped notice), and the length of the free ends of the oblique. Evidently we have here a sufficient number of independent variables to account for the most radical divergence of opinion and explanation among observers who fail to treat these elements in an identical manner. The case affords an admirable instance of the futility of supposing that a single principle of explanation will suffice for every illusion which chances to seem simple.

It is to be regretted that the author has not seen fit to add a chapter correlating the results of the experiments on visual illusions with one another and with the discussion of auditory space. Meantime, it is a pleasure to acknowledge the solid merit of the work, which is a thoroughly creditable addition to the experimental psychology of our day.

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Nouvelles Observations sur un cas de Somnambulisme avec glossolalie. TH. FLOURNOY. Archives de Psychologie de la Suisse Romande, I., no. 2, pp. 102–255, December, 1901.

Professor Flournoy devotes another 150 pages to the case of Mlle. Hélène Smith, the heroine of the volume 'From India to the Planet Mars,' which was issued about two years ago and somewhat later appeared in an English translation (see notice in the PSYCHOLOGICAL REVIEW, July, 1900). It cannot be said that Professor Flournoy has very much new to offer; but there are some points of interest worthy of record. It appears that the fortunes of Mlle. Smith have changed during these two years. Professor Flournoy describes in four stages this change since the publication of the former volume. The first is a stage of irritation and annoyance at the many criticisms to which she as the subject of the volume was subjected. She seemed to feel more deeply than when the several records were submitted to her, the serious difference of opinion between Professor Flournoy and herself, and in a measure to hold him responsible for any adverse or ungallant comments which found their way into the press. In brief, in connection with a growing tendency on the part of Mlle. Smith to believe in the inspired and supranormal nature of her trance revelations, and to feel the sharper contradictions between such a position and that seeking a common-sense explanation, there grew up social and personal compli-

cations which led to Professor Flournoy's degradation to a position of *persona non grata*. In the second phase, Mlle. Smith seems disposed to make amends for this condition of affairs and to recognize that M. Flournoy was really responsible for the celebrity to which she had attained. She accordingly sent him an account of some new revelations so that he might have some interesting developments to present to the approaching Congress of Psychology at Paris. The third stage M. Flournoy calls, though he admits that the designation is 'un peu barbare,' the American stage. It indicates that Mlle. Smith was besieged by a company of zealous admirers all anxious to have sittings, to see the wonders of her trance conditions and to consult her by way of messages from the other world, and so on. The fact that many of these were American ladies is the basis of the designation. Mlle. Smith throve in the sympathetic atmosphere thus provided, and her automatic life continued to blossom and send forth new branches. By the munificence of a lady of wealth, Mlle. Smith was relieved of the uncertainties of earning a living as an employee of a shop at Geneva, and was thus enabled to devote herself more uninterruptedly to her psychic experiences. In the present stage Mlle. Smith has been visiting in Paris, has gathered about her a more or less sympathetic company of believers in her spiritualistic and supranormal powers, and as a consequence much of Professor Flournoy's information is at second hand, or in the form of letters. Yet he is not altogether banished from the medium's favor, though he must be content to receive and not to direct the current of inquiry. It is hinted, too, that Mlle. Smith is herself going to turn author and publish her own spiritualistic account of her story, using the material which she refuses to place at the disposal of M. Flournoy.

Following the Martian revelations which were detailed in the former volume, the medium removed the scene of her visions to a further planet, which is here designated as ultra-Mars; and here again we have descriptions of scenes and peoples, occasional sketches, a new language of strange sounds though of French structure, as was the Martian tongue. Pages of the texts are given, as are also samples of the scenery and interiors of the ultra-Martians; and they are more or less childish in their attempts to suggest the bizarre and unearthly. Worthy of note are the ultra-Martian ideograms or set of symbols by which the words are presented. These are about as different and as perplexing as the characters of Chinese; and the fact that they are consistently employed constitutes a notable feat of the subconscious memory of Mlle. Smith, and the more so since this is done for more

than one of these fictitious languages. But there are further worlds beyond Mars; and the next trance cycle takes place on Uranus, from which in turn the scene shifts to the moon. Again a curious geometrical alphabet, different from the others, and yet for brief passages consistently maintained; again descriptions of strange scenes and the usual accompaniments of her remarkable imagination. The evidence thus furnished of the continued growth of these somnambulic creations is indeed noteworthy, and gives a more profound sense of admiration for the subconscious mentality than one is likely to have held before. Still more important is it to note that the genesis of these cycles follows one genetic course. They all begin, so far as the linguistic part is concerned, with the subject's hearing a few words of the unknown tongue, and later on repeating them; still later she sees the characters, and finally is able to reduce them to paper by automatic writing. Leopold, her spirit guide, takes a variable part in the translation. There are always considerable intervals between the stages of such a cycle; these M. Flournoy calls periods of incubation or subconscious preparation. The languages and the celestial scenery are thus not a sudden revelation, but come only when the subconcious attention has been directed to them for a relatively long time. There are likewise further revelations of the Hindu cycle, in which the medium had produced a language with recognizable Sanscrit elements, and had become the incarnation of a former Indian princess. The Sanscritoid character of her linguistic invention is further established; likewise the complete historic anachronism between this speech and the character which the medium is supposed to be personating, for the latter in real life knew no Sanscrit. The origin of the information which Mlle. Smith possesses in this cycle is not clearly set forth, but enough is shown to make it evident that a retentive memory would have found the data in various incidents accessible to her reading and experience.

The royal cycle in which the medium becomes Marie Antoinette is likewise not neglected, and is naturally aroused by her visit to Paris, where indeed she enacts some of her trance scenes amid the original settings of the unfortunate queen. Here too, a historical character is introduced with very unhistoric concomitants, Dr. Barthez, a physician of the court of that period.

There are further evidences of the continued activity of the subconscious and its connection with the emotional life of Mlle. Smith. The efflorescence of the Marie Antoinette cycle under the inspiration of a visit to Paris is a case in point. The fact that upon learning the news of the fortune presented to her by her benefactress she experienced a

vision of Leopold, is another. She was about to step on a tram-car to bring the good news to her mother when Leopold appeared on the step of the car, forced her back, and conducted her through the crowded streets to the shop of her employer, whom she then bade farewell. It is also interesting to note that this subconscious faculty may be put to some use. On one occasion Mlle. Smith thus recovered a piece of goods which she had mislaid or had witnessed some one else lay aside; and on another occasion she recovered a similar lapse of memory by an appeal to Leopold.

Perhaps the most striking part of this sequel is that it differs so little from the former chapters of the detailed story. The story still goes on, and in its continuance emphasizes the correctness of Professor Flournoy's diagnosis. The source of all this peculiar intellectual activity is the subconscious romancing imagination of Mlle. Smith. No other hypothesis is in the least adequate; and no other receives the successive corroboration which this one receives from the anachronisms and the details of the various revelations. The whole remains a noteworthy case of 'mediumistic' phenomena, and will serve for the edification and furtherance of, and not as an impediment to, the psychology of these phases of mental expression in so far as their connection with the normal mental life is appreciated and established.

JOSEPH JASTROW.

Ueber Begleiterscheinungen psychischer Vorgänge in Athem und Puls. P. ZONEFF und E. MEUMANN. Philosophische Studien, Vol. 18. Pp. 1–113.

This investigation divides itself into two parts. The first is a study of the relation between the various forms of attention and the changes in respiration and heart rate, the other of the effects of pleasure and pain upon these physiological processes. The records were taken by means of the sphygmograph and pneumograph of Marey. Two pneumographs were used, one was fastened about the body between the first and second ribs, the other below the sternum, and each was attached to a framework of cloth in a way which permitted successive experiments to be made with the same adjustments. It was thus possible to compare the effect of the various stimulations upon the thoracic and abdominal breathing.

The forms of attention were so chosen that they covered the different senses. Among the tests were reading, counting the crosses upon a line, hearing out overtones, obtaining the least noticeable pressure by means of von Frey's hairs, and mental calculations. The ac-

companying effect upon the respiration was an inhibition. In the milder forms of attention this showed itself in the quickness and shallowness of the respiratory movements, while in the more profound forms there was almost a complete cessation of the movements. The intellectual was distinguished from the sensory attention by its more pronounced effects. The pulse was uniformly slowed during the attention process.

Pleasure and pain were induced by colors from gelatine plates, by consonant and dissonant tones, by tastes, and by memory images both simple and complex. The investigators confirmed the classical statement that the effects of pleasure and pain upon both breathing and pulse were exactly opposed. The respiration was quickened and more shallow, the pulse slowed, during pleasure, while respiration was slow and deep in pain, with quickened pulse. The thoracic breathing was always most affected, and the abdominal was either not changed at all or showed a change in the opposed direction from the thoracic.

In addition to repeating more carefully these fundamental experiments, the authors obtained interesting results upon several related problems. The first was as to the relation between the attention efficiency and the corresponding physiological processes. Records were taken while the reagent was endeavoring to hold a copper wire just above a metallic plate. It was found that the greatest number of contacts were made at the time when the bodily processes were least characteristic of active attention. It is suggested that this indicates that the fluctuations of the attention may be traced to its physiological side. There is not mentioned the probability that both the changes in pulse and respiration rate and the attention fluctuations may be due to the rhythm of the vaso-motor center.

Another statement of the text-books, founded upon self-observation, which was investigated was the effect of directing the attention to the feeling process itself. The usual statement that attention to the feeling destroys the feeling was not found to be true of the effect upon the physiological concomitants. On the contrary, the effects upon both respiration and pulse are increased. The authors however state that they think the result would have been different if psychological introspection had been attempted instead of mere attention to the feeling.

Distraction of the attention destroys the effect of the feeling, whether of pleasure or of pain. The result is always the characteristic of the attention alone.

The experiments as a whole seem conclusive with regard to the opposed effects of pleasure and of pain on respiration and pulse. It is much to be desired that some one carry on a similar investigation upon the effects on body volume. W. B. PILLSBURY.

UNIVERSITY OF MICHIGAN.

An Analytic Study of the Memory Image and the Process of Judgment in the Discrimination of Clangs and Tones. G. M. WHIPPLE. American Journal of Psychology, Vol. XII., No. 4, 1901, pp. 409–457.

The aim of this investigation was chiefly to determine how much influence the memory image of a tone has upon its discrimination from another tone. Reed tones as well as bottle tones were used. Three of the six observers were musical, three unmusical. The reed tones used as standard tones were of 612, 724, 832, 928, and 984 vibrations, the variations about 8 vibrations. The observations recorded are highly interesting from more than one point of view. Only a few can here be mentioned. Increase of the time interval caused a general decrease in the total number of right cases. This, however, can not be explained by corresponding changes which the memory image may be supposed to undergo. On the contrary, the memory image was often found by introspection to be exceedingly weak or entirely lost when the judgment was certain and correct. Occasionally a good image may be present without insuring a decision. Sometimes reports were given like this: *V* was way below my image, yet I felt compelled to judge 'equal' on account of some feeling of familiarity. The practice effect is almost entirely confined to the unmusical observers. Within the octave employed there is no observable dependence upon pitch. Writing down the judgment, or repeating the tonal interval several times, often brings on a feeling of certainty which was not present when the judgment was made. The unmusical observers often judged 'different' without deciding whether lower or higher. The musical observers almost never confused the direction of a difference. In one case light was thrown upon the mechanism of judgment as conditioned by what might be called ease of reproduction: "I knew the difference easily, but not the direction; said lower because it is easier to say." If *V* differs from *N*, the process is not, as a rule, felt to be so largely auditory; the attention is entirely taken by a complex something which stands for high or low. What the something is depends upon the individual; its core is usually a complex of strain sensations, its remoter elements visual or

organic. If *V* fails to engender either the reaction of familiarity or of specific difference, the observer resorts to auditory comparison. The resulting decision is usually uncertain and very apt to be incorrect. Pleasantness is the correlate of 'certain' judgments, not of any of the categories equal, higher, or lower. The verbal formulation arises only after the decision has been made otherwise. Speed, certainty, and correctness of judgment are closely correlated. In experiments with distraction during the time interval by means of odors, it was found that distraction slightly lessens the total number of right cases for all observers, though, it may be added, this effect is not due so much to the loss or impairment of the image as to the time consumed by the shift of 'venue' at the moment *V* sounds; *V* fails to 'sink in' at first if the distraction be complete. MAX MEYER.

UNIVERSITY OF MISSOURI.

SOUL PROCESSES.

Psychische Vorgänge und psychische Causalität. TH. LIPPS. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Vol. 25, pp. 161–203.

This absorbingly interesting paper is occupied with the proof and elaboration of the two following theses: (1) Conscious states can only be related to one another indirectly or through their connection with the unconscious soul-processes (*psychische Vorgänge*) in which we must assume them to be grounded. (2) Conscious states as such are absolutely passive and have no power to modify the working of their unconscious substrates.

Professor Lipps believes that the aim of psychology should consist not merely in describing mental phenomena but in making their relations intelligible. Just as the physicist in order to reduce physical facts to causal order finds it necessary to supplement the objects of immediate experience with conceptions of ether and atoms, so also the psychologist is forced to infer, as the direct basis of the fragmentary series of subjective conscious contents, a continuously existing order of real things, that is, things which can be objects of thought but which in no way depend for their existence upon our being aware of them. We must, for example, posit a soul, first, as the substrate of the immediately felt subjective unity or I aspect of each psychosis, second, as the bearer of the system of thought-traces and dispositions which are bound up with the I. And the same reasons which make it necessary to infer the existence of a soul as the substrate of the unity of our conscious life, make necessary the further assumption of real

processes or occurrences in the soul as the substrates of the specific and relatively differentiated states of consciousness. Professor Lipps does not undertake a detailed classification of these soul processes or *psychische Vorgänge*, but from what he does say we gather that he regards each form of *sensation* as grounded in a distinct soul process, while *representations* (*Vorstellungen*) may be adequately described as grounded in complexes of the sensation-substrates. As for the rest of our conscious states, particularly the 'feelings' of time and space relations and of similarity and contrast, they are to be thought of somewhat in the Herbartian manner as resulting from the dynamical relations of the sensation-substrates to one another and to the soul in which they occur.

The strongest reason for believing in these *psychische Vorgänge* is that without them it is impossible to speak of psychical causality—or, indeed, of any other real relation between conscious states. For suppose we try to disregard the real grounds of conscious states and think simply of what we are conscious of. A conscious content regarded merely as such is purely ideal and can have only ideal relations to other contents. Someone may think of one content as causing another, but, needless to say, it is not people's thoughts of psychical causality but psychical causality itself that the psychologist seeks to determine. And in the same way with the pure time relation: a conscious content *as such* cannot precede or follow anything, for as a mere ideal content it has no place in a real time series. In short, Professor Lipps seems to feel that a content considered as such, that is, as purely ideal, cannot stand in any real relation, for the same reason that an ideal dollar cannot pay a real debt. If conscious states are to stand in any real relation, it cannot be due to any peculiarity of content. To what, then, can it be due? Obviously, to the only thing that we can connect with a conscious state except its content, *viz.*, its source or ground in a soul process. Not *what* we are conscious of, but *that* we are conscious of it, makes it possible to speak of a conscious state as standing in real relations.

On this very subtle and, as it seems to me, for the most part convincing argument Professor Lipps bases his plea for the *psychische Vorgänge*. He then proceeds to show that his view is in harmony with the naïve realism of common sense, and also to some extent with the attitude of the materialistic physiologist. For the 'plain man,' if asked whether the qualitative content *a* calls up the second content *b*, will reply that it is rather his *perception* of *a* which calls up his perception of *b*. As for the physiologist, he too will insist on the fact that the

perceived sequences of consciousness do not explain themselves but are explicable only by assuming an interconnected system of real processes underlying them. But because we can agree with the physiologist thus far, we are not obliged to concur in his further assumption that the direct grounds of conscious states are processes in the brain. For "the processes which are started by an external stimulus tend to bring into existence a sensation-content, and on their way to this goal they pass through a sphere in which they are brought into interaction with one another and also with concurrent processes which have originated in that sphere without the agency of an external stimulus. This sphere is distinct from the sphere of conscious life; it is the sphere of the real psychical occurrence. 'In the soul' [*Psyche*] means 'In this sphere'" (page 180). We must, moreover, be careful to note that the *psychischer Vorgang* never *becomes* a conscious state, it only tends to introduce one. When the *Vorgang* attains a sufficient degree of intensity, a sensation content '*steps in*';—just how or why this happens is the great riddle.

Having thus established the existence of the *psychische Vorgänge*, Professor Lipps passes to the justification of what we have called his second thesis. Consciousness has no effect upon the *psychische Vorgänge*. The unconscious soul-life with its complicated interactions and transformations is no more affected by the conscious contents which a portion of it occasionally brings about, than the waterfall is affected by the rainbow that plays now and then over its surface.

The arguments offered by Professor Lipps in support of the 'epiphenomenality' of consciousness are both general and special. The general argument is most explicitly stated on pages 171 and 185. It consists in the assertion that real objects and real relations are never matters of immediate experience but are entirely the result of inference; for the only thing presented in immediate experience is the purely ideal content, which has, as such, no place in a real temporal or causal order, and is hence necessarily without influence on the *psychische Vorgänge*. Professor Lipps, if I have caught his meaning, regards the truth of this statement as the self-evident result of introspection. Look into your mind and you will see there nothing but ideal contents. The 'real' is a pure inference and as such is always unconscious. We venture to think that this would not be accepted by most people as an adequate account of what they immediately experience. Introspection shows us not merely ideal contents in purely ideal relations but ourselves actually perceiving those contents. *That* we perceive is no less truly a datum, than *what* we perceive.

It is, indeed, necessary to acknowledge that we have an immediate consciousness of the real, if we are to believe in a real world at all, for, as Professor Case has so forcibly shown in his book entitled 'Physical Realism,' it would be quite impossible to frame by mediate inference a conception of something of whose nature we had had no inkling in direct perception. We could no more conceive of reality if we had not felt it 'up against' us in every sensation, than a man born blind could conceive of color.

But perhaps an even more serious objection to Professor Lipps's general argument for epiphenomenalism is its apparent inconsistency with his belief in the power of real processes to bring about conscious states. If there is sufficient homogeneity between the real and the ideal to admit of the *psychische Vorgänge* causing the conscious contents to 'step in,' then it seems hardly justifiable to deny that that same 'stepping in' or becoming conscious could react upon the unconscious conditions which produce and sustain it. If one-way action is a fact, interaction is at least conceivable.

Professor Lipps is, however, by no means content with urging general considerations in support of his second thesis; he examines the phenomena of attention, association and analysis (where, if anywhere, we should expect to find mental processes influenced by conscious contents), and undertakes to show that in every case it is the unconscious process which guides and conditions the conscious without being in any way reacted upon. In this part of his paper Professor Lipps certainly gives us a remarkable demonstration of the utility of his theory in throwing light upon the more obscure phases of attention and association. One feels that the author's ideal of making psychology an explanatory as well as a descriptive science is by no means impossible of realization, and that the *psychische Vorgänge* have a right to exist, if not as demonstrated verities, at least as a very valuable working hypothesis. When we consider, however, that the nominal purpose of this portion of the essay is the disproof of the efficacy of the conscious and only indirectly the justification of the unconscious, we are inclined to feel that the special arguments to this end are as unconvincing as were the general. In the treatment of attention, for example, we are made to see very clearly that the content to which we attend can not draw our attention merely in virtue of its own ideal nature. The 'thought-traces' and dispositions which are the soul's record of past experience are acting upon one another in such wise as to make possible the attention to the present content; but granting that the chief determinants of our acts of conscious attention are the

unconscious inhibitions which the *psychische Vorgänge* exert upon one another, it by no means follows from this that the nature of the conscious content may not also affect the attention. Again, in the case of association, Professor Lipps succeeds in proving that the associative tie between similar contents is due not merely to their similarity but to the fact that the soul processes which underlie them are linked together. But he does not, as it seems to me, prove the further and much more radical thesis, that the feeling of similarity, or the unconscious basis of that feeling, is entirely uninfluenced by the actual similarity of the contents. Is not the fact that we perceive two triangles to be more similar than a triangle and a circle due in any degree to the fact that as conscious contents they actually are more similar in so far as they contain a greater number of identical qualities? Or must we hold to the paradox that the similarity which we think we perceive between conscious contents is a pure delusion due entirely to the unconscious *Vorgänge?* And, thirdly, in the case of analysis, Professor Lipps, if I understand him, attempts with equal unsuccess to prove that when we think we perceive and analyze a complex conscious content, we are really perceiving a content that is quite simple, the delusive feeling of complexity being brought about by the interactions of the several sensation substrates which combine to occasion the representation-content.

Underlying the arguments both general and special which Professor Lipps urges in support of his second thesis, is the strong conviction on his part that it follows as a corollary from the first thesis. The efficacy of the unconscious *means* the inefficacy of the conscious. Now if we think for a moment of the grounds which justify the first thesis, I believe we shall find that it is the falsity of the second thesis rather than its truth which is implied. The real basis for assuming a soul and soul processes is after all simply one case of the need of postulating *continuity* between the disparate members of a single system. The motions of the brain molecules are certainly bound up in one system with the intermittent series of conscious states which make up our experience, while it is equally certain that they are disparate. Two courses seem open. We may, with the materialist, regard the permanent ground of our recurring experiences as the brain, and define thought as a mode of motion. Or, on the other hand, we may, with the transcendentalist, take the timeless ego or unifying principle of consciousness as the real self and define brain motions as a mode of thought. The objection to both theories lies in the fact that consciousness and motion in space are so disparate that neither can be properly

conceived as a mode of the other. We cannot help matters by listening to Hegelisers who would deftly explain the relation of thought and brain motion by the platitude that they are 'abstract aspects of a concrete organic unity,' for it is precisely the fact of an organic unity of disparates that calls for explanation, and the statement of the problem can scarcely be accepted as its solution. Now when confronted with this sort of problem in the past, science has always adopted one course; knowing that continuity must exist between two related elements, and not being able on account of their disparateness to express one as a mode of the other, it has posited a *tertium quid* as homogeneous with both elements and hence the basis of their continuity. The soul with its processes or *Vorgänge* is thus to be posited as the medium by which brain motions occasion the consciousness which reflects them. If this is at all a fair statement of the matter, it would seem clear that Professor Lipps is directly at war with himself when, after positing as the basis of the conscious *Ich-gefühl* a similarly simple real soul whose *Vorgänge* are continuous with physiological stimuli, he proceeds to deny that these intermediary processes are in any degree homogeneous with, or modifiable by the conscious contents which they are admitted to occasion.

But Professor Lipps's article is interesting not so much for its possible failure to prove the epiphenomenality of consciousness, as for its success in proving the *psychische Vorgänge* and in applying them to special cases of psychical causality. His hypothesis seems, as we have already said, to be a very real step toward making psychology an explanatory science by raising its empirical laws to that plane of *Anschaulichkeit* or quantitative continuity which according to Kant is the goal at which every science should aim. To this goal the physical sciences are fast attaining, and, if we may trust Professor Lipps, there is no reason why psychology alone should continue to be burdened with the categories and methods of obscurantism. The paper will be welcomed by all who sympathize with the Kantian ideal of scientific method and with Herbart's fine confidence in the applicability of that ideal to the phenomena of mind.

W. P. MONTAGUE.

UNIVERSITY OF CALIFORNIA.

A History of Modern Philosophy. DR. HARALD HÖFFDING, Professor at the University of Copenhagen; translated from the German edition by B. E. MEYER. London, Macmillan and Co. 1900.

There are several points of view from which a review of such a work as this may be written. The critic may regard it as his func-

tion to indicate the completeness or incompleteness with which the author has succeeded in gathering up and displaying the philosophical opinions of the greater and the lesser lights that have an admitted right to be enumerated among the philosophers; he may dwell upon the skill, or the lack of it, with which the genesis of successive systems of thought has been traced, and the sense of proportion which is exhibited in the space allotted to the discussion of schools, of individual men, and of opinions; he may criticise the bulk and the exactitude of the author's information, the thoroughness of his acquaintance with the original writings which constitute the true history of speculative thought, and the keenness of his insight into the real meaning of passages whose obscurity has been a stone of stumbling to many generations of students; he may point out the originality with which the matter which is presented has been set forth, and may congratulate the author upon his gift of literary expression, not forgetting to abuse the translator for this or that blunder in the rendering of passages which have lost their true character in putting on another dress.

But Professor Höffding's excellent work has been for some little time before the public. It has received a good deal of criticism of this sort, and it is not my intention to add to it. I wish to dwell rather upon the purpose which he has held before him in the production of these volumes, and to indicate the significance of his method for the study of philosophy, not as literature or biography, but rather as science—science in the broad sense of the word, which does not limit its use as it is too often limited by the unthinking.

Dr. Höffding points out with justice that it is wise to devote much attention to the history of philosophy. In this field the past cannot be ignored as it seems possible to ignore it when one is cultivating many scientific disciplines. There is no established body of doctrine which we have a right to impose upon the student as authoritative. He who would attain to a just estimate of the problems of philosophy as they present themselves to our age must approach them with a mind enriched by a knowledge of the forms which they have taken in the past, the shapes which have been given to them as a result of the labors of the great reflective minds of the race. Only then can he hope to distinguish between the accidental and fleeting, and the relatively permanent; between the passing prejudice or misconception of a day, and that which may turn out to be a true insight into the nature of things and worthy of being regarded as a contribution to scientific truth. Such historical studies reveal that several factors influence the treatment of the problems of philosophy. Dr. Höffding recognizes the following:

1. The personality of the philosopher. As we are not in the realm of exact knowledge, we may expect the personal equation to be large, and, in fact, we do find that it is very large.

2. The observations which are taken as a basis for the construction of a philosophical system. For example, the development of the natural sciences has been of the greatest importance for modern philosophy. Its leading problems seem to be determined by the fact that these sciences have arisen. Historical circumstances, and intellectual movements of many sorts, may be seen to be responsible for the turn which philosophic thought has taken at many a critical point in its history.

3. The consistency with which initial assumptions are laid down and maintained, and their logical consequences deduced from them.

The exposition and criticism of the systems treated of in the two volumes before us turn upon these three points, but these points are not given an equal amount of attention. The author attaches most weight to the first two. He thinks that inconsistency in a great thinker 'is often nothing but the natural consequence of the fact that his genius displays itself in *several* lines of thought, although he may not himself have been able to follow these out far enough to discover their mutual contradiction' (Introduction, p. xvi). He regards it of importance that each of these lines of thought should be developed, while freely admitting that it is desirable that depth of thought and consistency should be united.

That the position taken by Dr. Höffding in his introduction is not belied in the body of his work, a careful examination of his volumes will reveal. It should be observed that this makes his book rather a history of human culture, presented in the form of a series of spiritual biographies, than a critical history of philosophy written with a strictly scientific aim. I do not, of course, mean to maintain that a recognition of the personal equation may not be of value in philosophy as elsewhere, nor that a knowledge of the historic background of a system may not help us to understand the real significance to the writer of statements which, to a man in a different intellectual environment, must remain obscure. But I do mean to point out that it is one thing to discover how a man came by a thought, and quite another to estimate its value as a contribution to human knowledge. If, in writing a history of philosophy, we devote our attention largely to the former, our work is anthropological, historical, biographical, but it is scarcely philosophic. If we take philosophy seriously, and make it our first aim to attain to a clear and unprejudiced view of the problems with

which it concerns itself, we shall not overlook the personal equation and the historic setting of a philosophical system, but we shall regard any attention paid to these as merely preliminary, as a something wholly subsidiary to the most important part of our investigation. The problems presented to reflective thought are not new. There has been no such general and indubitable progress in reflection that we are justified in approaching the philosophy of a Plato or an Aristotle as merely matter for interesting historical investigation. The absorbing question is whether the initial assumptions which it seemed to such a man reasonable to lay down may be accepted, in part at least, by ourselves. In weighing such assumptions, we must be influenced, not merely by their seeming agreement or disagreement with the facts of experience as we see them, for we, as well as those who have preceded us, may misinterpret our experience; we must be influenced also by a realization of the consequences which seem to follow from such assumptions, and we may find that the men of the past have traced such consequences with great labor and ingenuity. By this labor and ingenuity we may profit; and he has not labored in vain who has shown that a road which many may be tempted to travel, leads to no desirable goal or ends in an *impasse*.

I feel, therefore, justified in entering a friendly protest against the conception of the history of philosophy embodied in these volumes. It concerns itself, it is true, with what men have thought and how they have come to think it; but it does not sufficiently recognize the great importance of the distinction between coming to think a thing for no better reason than that it appeals to one's psychological idiosyncrasies, or is suggested by this or that book or by this or that social or religious movement, and coming to think it because one's genius for reflection enables one to see with clearer vision than is granted to most men the problems which human experience lays before us, and the possible solutions which may be found for them. Moreover, it should not be forgotten that the full meaning of a conception as it presented itself to a philosopher of constructive genius may be very inadequately portrayed if we depend largely upon an indication of the sources from which the man has borrowed, and overlook the fact that it may mean to him much more than it did to those from whom he has taken it. What it really meant to him can only be determined by the most careful examination of its place in the man's own system of thought. The fact that other men have had a more or less similar conception, and have used the same word to represent it, may be positively misleading. Neither Aristotle, nor Augustine, nor Thomas, nor Bruno, nor

Spinoza, nor Kant, coined for himself, by a free act of intellectual creation, the conception of God. Each appropriated something that he found ready to hand, and he appropriated also a word in common use. Is it possible to attain to a just estimate of what the word meant to any one of these thinkers if we pay attention chiefly to his psychological peculiarities and to the materials which he inherited and with which he was compelled to begin his construction? Must we not come back to his own system, and strive to obtain a comprehension of its real structure? In other words, must we not dwell upon his initial assumptions and the consequences which he found it possible to draw from them? If we assume that every time a philosopher uses the word God he means what was meant by those who preceded him when they employed the same word, we are in danger of falling into an error analogous, at least, to that into which we should fall did we assume him to mean what we mean when we use the word.

The defect of method upon which I have dwelt in the preceding pages does not, of course, make itself equally apparent in all parts of Professor Höffding's work. There have been ages in which men have borrowed largely from the past and have not greatly modified the content of that which they have borrowed. There have been times when the broadening and deepening of existing conceptions have seemed to be the inevitable result of a rapid development in the field of natural science. There have been eclectics whose opinions can be well understood when we take into consideration the two factors upon which Dr. Höffding mainly dwells. Any man can read with profit his fine chapters upon 'The Renaissance and the Middle Ages' and upon 'Humanism.' Nor can one complain of his treatment of such a writer as Montaigne, who was not a man of constructive genius, nor one whose opinions form a whole each part of which loses much of its significance when divorced from the rest. But it is a different matter when Professor Höffding treats of such a thinker as Bruno.

When one has pointed out the profound significance of an abandonment of the mediæval homocentric world-scheme and of the adoption of the Copernican system with what seem to be its implications and suggestions; when one has dwelt upon the antagonism of the newly-liberated mind to the Aristotelian doctrine that had long held it in bondage; when one has traced certain conceptions and forms of expression to the influence of the Platonic doctrine, or to the speculations of Cusanus or Telesio; it does not follow that one has made plain to the reader the actual course of Bruno's argument in his most

important work, the dialogues 'Della Causa, Principio ed Uno.' Here Bruno has an argument; he does not merely express opinions. He argues loosely, he more than once changes his point of view, he depends upon quibbles which cannot easily be understood by one who is not familiar with the scholastic distinctions against which Bruno rebels; but he always argues, and it is perfectly possible for the patient student to attain to a comprehension of the transitions in his thought. There is, however, no other way of attaining to this than by a careful examination of his processes of reasoning. It is of little value to sum up his conclusions in general statements; to tell us that 'at every single place, in every single particle, the world-soul works as a totality.' A man may hold this perfectly well without being a Bruno at all, and without even comprehending Bruno's argument to show *how* such a thing can be possible, and what can be meant by this dynamic ubiquity. One may read and reread Dr. Höffding's discussion of Bruno's 'Fundamental Philosophical Ideas' (Vol. I., pp. 130–139), and yet obtain no clue to the rhapsodic utterances which compose the latter part of the third dialogue. Yet he who does not possess such a clue does not know Bruno. He is in danger of making him too modern a man; that is, he is in danger of making him what he was not.

A similar criticism may be made touching the treatment accorded to Spinoza. I know of no book likely to be studied by those who occupy themselves with the history of speculative thought which presents more difficulties than the 'Ethics.' To most readers it is practically a sealed book, so far as the actual structure of Spinoza's thought—the true course of his argument—is concerned. They gather from it certain luminous ideas, and they certainly read it not without profit, but they rise from its perusal with the vaguest possible notion of the bony structure, if I may so speak, of Spinozism. There are many pages in which the course of the argument is not apparent, and which they have followed with uncomprehending eyes. Professor Höffding's exposition of the 'Fundamental Concepts of the System' (Vol. I., pp. 307–314) will not give such pages a meaning, nor will any account of the life of Spinoza or of the sources of Spinozism take the place of a clear and systematic exposition of the course of the reasoning which unfolds itself in the 'Ethics.' I ask anyone who doubts this statement to see for himself whether anything that Höffding says will afford a clue to the meaning of such passages as the following: 'Ethics,' I., 5; Epistola, L.; 'Ethics,' I., 8, schol. 2; 'Ethics,' I., 13, and I., 15, schol. And yet the comprehension of these passages and

such as these is of the utmost importance to the comprehension of the conception of immanence and its place in Spinozism, which is much the same as saying that it is of the utmost importance to the comprehension of Spinozism itself.

I have chosen to speak of Bruno and Spinoza because it is in treating of such thinkers as these that Professor Höffding's method of writing the history of philosophy seems to be at its worst. I have felt free to dilate upon what I regard as a defect, and to pass over many excellencies, because the merit of the work is incontestible, and is generally admitted. Within the limitations made necessary by his method, Professor Höffding has written an excellent book, learned, clear, interesting and sympathetic. I have read it, and I shall again read it, with pleasure. But one may say all this and still hold that it is a book of one class rather than of another—a book for the general reader who wishes to know in a general way the opinions which men have held and to understand how they could come to hold them, rather than a book for the serious student of philosophy, to whom biography and history are, after all, matters of minor importance, though they are matters which the accomplishment of his purpose makes it impossible for him to leave out of account. There can be no doubt that the book is the more readable for being what it is, and that it will appeal to a larger circle than it would were it written upon a different plan. For one thing, the somewhat superficial treatment accorded to the doctrines of many philosophers makes it possible to mention, at least, a large number of men who would have to be omitted altogether in a work no larger than this one which attempted to treat philosophical systems in a thorough and analytic way. Of such men one likes to know something, and one is apt to feel defrauded when they are passed by in silence. Professor Höffding's work fills a place, and an important place, among books which treat of the *placita philosophorum*. I sincerely hope that many will read him, and that the *leves gustus in philosophia*, which he affords the reader, will lead some on to the *pleniores haustus* which must be sought for elsewhere.

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Ethics Descriptive and Explanatory. S. E. MEZES. New York and London, The Macmillan Company. Pp. xxi+445.

Professor Mezes' work is, I think, the most important, if not the first, systematic 'attempt to construct a positive or purely scientific theory of ethics, and to give a naturalistic account of all the aspects

of morality and immorality' which has been produced in this country. His sane and modest preface is well calculated to ensure the sympathetic attention of readers who diverge most widely from some of the positions which he maintains. He conciliates metaphysical moralists at the outset by the statement that while undertaking to write a scientific account of ethics, he does not undervalue or fail to realize the need of a metaphysic of ethics. There are in fact, he says, two distinct questions, viz.: What is morality? and, What is the cosmic significance of morality? The former question, to which Mr. Mezes confines his attention, 'can be answered quite as scientifically as the question, What is a living being?' The latter question is left for metaphysicians to handle; and there can be no serious objection to this division of labor. It is a somewhat unusual merit for 'scientific' moralists to recognize that there are these two distinct questions, and that the answer to the former of them does not of itself furnish an answer to the latter also.

Nevertheless, Mr. Mezes, who has a perfect right to set the boundaries of the subject he wishes to treat, seems to me to take too narrow a view of the scope of ethics. "Ethical writers," he says, "do not in any proper sense judge conduct or issue pronouncements as to what is right or wrong. Their more modest task is to discover and record men's genuine judgments as to what is right and wrong, or in other words, to discover what men really mean by right and wrong." "In an ethical treatise, all statements of what is right, as distinguished from statements of what men hold to be right, are open to distrust" (p. 7). Thus in spite of the fact that the author regards ethics as a normative science, he would really seem to limit it to the function of description—only, instead of describing 'conduct actually performed,' it is the function of ethics to describe 'an ideal type of conduct, actually so conceived by men,' or in other words, to state 'what men hold to be right.' But a science which simply records what men think, is just as much descriptive and just as little truly normative as one which records what men do. No doubt the knowledge of what kinds of conduct men are fairly agreed in regarding as right, will help the moralist towards an adequate conception of the norm, conformity to which constitutes right conduct. But the mass of mankind's intuitive ideas of right and wrong conduct are formed without any conscious reference to the ideal or normative end. And ever since the time of Aristotle a long line of representative moralists have held that it is the chief function of ethics, by reflection upon the nature of man, to determine the supreme end or good of human life. Mr. Mezes' objection

to defining morality in terms of an ideal end to be realized, or, as he puts it, the objection to designating ethics a 'teleological science,' is that this would be to assume that "it is bound to describe moral actions in terms of the result or end that they realize"; but since Kant and others hold otherwise, such an assumption is not warranted at the outset, however true it may prove in the sequel (p. 18). This caution, based upon the desire to give a definition which is broad enough to include all types of ethical theory, and not to anticipate results in such a way as to limit the scope and method of the science unduly, is commendable.

But there is some danger, on the other hand, of too narrow a conception of the function of ethics as a normative science. To say that ethics as a normative science is called upon to describe the standard actions that deserve to be called moral (p. 8), is not precisely the same thing as to limit it to the rôle of describing the actions which happen to be so called. Conduct may be held to be right for one of two reasons: either it is right in itself, without reference to the end to be attained, or it is right according as it furthers the Supreme End. If the former be true, then, in so far as men *know* what is right, a statement of what they hold to be right would at the same time furnish the norm of conduct. Thus ethics as a descriptive or historical science would at the same time be normative. But men admittedly are often mistaken as to what is right. While if right conduct be defined with reference to some supreme end, and men are not always agreed as to the nature of this end, and are often mistaken as to the best means to its attainment, *i. e.*, do not know what is right (and Professor Mezes emphasizes the wide divergence of opinion as to what is right), then the record of men's opinions as to what is right conduct, even if this could be said to constitute a normative science, would not, in my opinion, exhaust the scope of ethics. The adjective 'normative,' as currently used, is a question-begging word to which it is not well to attach too much significance. The important question is in regard to the nature of the problem with which ethics deals, and the reviewer differs from the author in holding that ethics does establish a norm or standard and judge or evaluate with reference to it, and does not simply record men's judgments as to what is right or wrong (7).

The first problem of ethics is to determine the norm, by which we mean not simply the description of a moral standard 'actually so conceived by men,' but a determination of the validity of the ideal; and the second problem is to determine, by reference to this norm,

'what right and wrong, morality and immorality respectively are.' In short, we wish to know not simply what men hold to be right, but what *is* right; and if ethics is incapable of satisfying this demand, there should be some other science—say metaphysics—whose function it would be to deal with the validity of the moral ideal and the reality of the distinction between right and wrong which is implied in our common moral judgments. In that case ethics would not only not be an independent science; it would derive its strictly normative character from its alliance with metaphysics. If, 'in an ethical treatise all statements of what is right, as distinguished from what men hold to be right, are open to distrust,' we must either look elsewhere than to ethics for an answer to the ethical problem, or we must admit that there is no answer. Meanwhile, a very respectable body of thinkers have always held that it is the business of ethics to deal with this very problem, and until some science shall by mutual consent or *vi et armis* appropriate this territory, ethics is not called upon to surrender its prescriptive rights.

Nor, as a matter of fact, does Professor Mezes ignore the deeper ethical questions so entirely as his definition of the scope of ethics might lead one to suppose. In Chapter III., which deals with Perceptional Intuitionism and Subjective Morality, he recognizes that moral perceptions are only the raw material of ethical theory, and that some moral perceptions are not valid. The problem then is to determine what moral perceptions are and what are not valid. This question is partly answered by the distinction between subjective and objective morality, which Mr. Mezes very properly emphasizes. Actions are genuinely moral (*i. e.*, subjectively) when conscience approves them (53). But since some moral perceptions are not valid, action in accordance with conscience is not always fully and adequately moral (*i. e.*, objectively moral) (54). Every action that is moral at all is subjectively moral, while objectively moral actions not only follow their agent's conscience, but also the approval of the wise conscience, to which in such cases the agent's conscience conforms (55). It thus appears that the standard of objective morality is not what men hold to be right, but the conscience of the wise man. But who is the wise man? He is one who 'will in his actions serve the supreme end, and will in his insight view it,' though even he cannot describe it fully (382). Since, then, ethics refers us ultimately to the wise man for our knowledge of what is fully and adequately moral, and since the wise man is one who knows and follows the supreme end, which is given a fairly definite content by the inclusion of the cardinal virtues, I do not see why we

may not refer directly to this supreme end as the norm, and judge conduct by its conformity or non-conformity to this standard.

Fundamental to Professor Mezes' system is the distinction between subjective and objective morality, already noted; and since he holds that it is on a basis of subjective morality that the more difficult task of understanding objective morality can best be approached (55), he first treats of the former. The book is accordingly divided into two Parts: Part I. dealing with subjective, and Part II. with objective morality.

Voluntary action and conscience, which are the two parts of subjective morality (55), are the subjects dealt with in Part I. Only one chapter is devoted to voluntary action, because this subject is well treated in works on psychology. The chapter is slight and does not call for comment. Mr. Mezes apparently follows Professor James in his theory of the emotions and in his view of the rôle of effort in the moral life.

The discussion of Conscience, which occupies the remainder of Part I. and about a fourth of the book, begins with a chapter on the the Adult Conscience. Professor Mezes accepts the division of conscience into an emotional and an intellectual aspect, and then proceeds to describe the principal component emotions and ideas. The most important moral emotions are responsibility, obligation, free-performance, and feelings of approval and disapproval, the three former appearing only when the agent's actions are in question, the two latter generally appearing when the actions of others are regarded. The intellectual elements of conscience are ideas that awaken the emotions just named, *i. e.*, such ideas, namely, as justice, temperance, veracity, etc. Conscience, however, is not to be identified with obligation, remorse, or any of its components, but rather with the moral ideal. "The moral ideal is the dynamic, schematic system of such ideas of action as awaken approval or disapproval, responsibility, obligation, and the sense of free-performance" (90). In other words, we understand Professor Mezes to mean that conscience involves the consciousness of a practical ideal, together with a felt obligation on our part and that of others to live up to it so far as in us lies, successful effort in this direction calling forth moral approval, while failure or lack of effort is followed by disapproval. Professor Mezes' analysis apparently takes cognisance of all the essential elements of conscience, and is sane and sound; but it is surely unnecessary, and moreover unlike the author's usual directness, to resort to such a style of exposition as is illustrated in the definition just quoted, or in the following: "An ideal is an epit-

omized biography of an exemplar, or a composite biography of many exemplars acting in some particular capacity."

In Chapter II. it was shown that only voluntary actions are moral phenomena. But while no phenomena except voluntary actions awaken conscience, some of these fail to do so. The question accordingly arises: "What voluntary actions are and what are not morally judged?" This question is answered in Chapter VI., which maintains that conduct is the cause of conscience. Actions done from caprice, preference and prudence, though voluntary, do not arouse conscience. This is because the effects of such actions vitally and evidently affect only the agent and not others. Conscience appears only when the agent becomes aware that his action vitally affects the interests of others. Accordingly conduct, which is said to be the psychic cause of conscience, is defined as 'apprehended voluntary action that the apprehending subject views as affecting others in interests he considers vital' (98). Now Professor Mezes undoubtedly has the right to define the terms he uses; but aside from the fact that this seems an unusual limitation of the import of the word conduct, the position just stated seems alike contrary to fact and to the author's fully developed view. For, as he immediately remarks, 'it is generally recognized that each man owes duties to himself,' and that 'self-respect demands such and such conduct.' "Conscience is undoubtedly aroused when a contemplated action threatens to take from the agent's moral power and quality." Injury to the self of prudence does not arouse conscience, but injury to the moral self does arouse it (98, 101). The apparent contradiction between these two views is explained by saying that 'a man objectifies his moral self and looks upon it as an *alter ego*, and moreover that his moral self is really the representative of the interests of others, and, therefore, that in protecting the moral self conscience protects an *alter* and indirectly protects the interests of fellow men' (101). With the greater part of this statement we are in agreement. In no sphere has modern psychology labored more successfully than in showing the social origin and nature of the individual self. Nevertheless it is important to distinguish between the real relation between the individual self and the *alter ego*, and the conscious recognition on the part of the individual of this relationship. A relationship which psychology or metaphysics with much patient study discovers, is not necessarily present even to the reflective consciousness, and the statement that in order to awaken conscience a man must objectify his moral self and look upon it as an *alter ego* which represents the interests of others, is a very

questionable psychological proposition. Without resorting to a philosophical explanation of the relation of the individual to the social self in order to reconcile two apparently conflicting views, it is surely truer to fact as well as 'a simpler course' to adopt Mr. Mezes' alternative definition of conduct 'as action that is apprehended by the agent as vitally affecting other persons, quasi-persons, or his own moral self.' Even this definition of conduct is, I think, too narrow; but it has at least the advantage of making it easier to recognize character as an object of moral judgment. Voluntary actions are 'the indication and embodiment of character'; but if character is itself a moral phenomenon of supreme importance, the character of the agent is an important moral phenomenon, and is regarded by himself as, in Mr. Mezes' phraseology, a cause of conscience. It is only in virtue of the 'simpler course' of interpretation above mentioned that Professor Mezes can be acquitted of confusing the question of the psychic cause of the adult conscience, *i. e.*, 'what mental states have the dynamic efficiency necessary to arouse it' (91), with the question of the origin of conscience.

The ontogenetic question is treated in Chapter VII. on the 'Birth and Growth of Conscience in the Child.' Since conscience is made up of ideas of moral agency, the author is led to ask, first, How does the child come to get ideas of agents? and secondly, How does he get ideas of moral agents? (107, 118). Throughout the chapter the author pretty closely follows Professor Baldwin's exposition through the 'projective,' the 'subjective' and the 'ejective' stages, teaching that the child gradually learns to be an aggressive agent, a submissive agent, a social agent, and finally, through obedience and imitation, a moral agent.

The purpose of Chapter VIII. 'is to discover the conditions under which conscience appears and develops in the race.' There is general agreement that conscience is present in all men, and absent in all animals. "It may be, then, that conscience is one of the differences, or that it grows out of one or more of the differences between men and other animals." At all events, in attempting to distinguish between moral and non-moral phenomena, we are confronted by the fact that the actions of men are commonly regarded as moral phenomena, while the actions of brutes are not so regarded; hence we are led to inquire what are the differences between men and other animals in virtue of which the former are pronounced to be moral agents and the latter are not. The mental differences which Mr. Mezes gives are speech, judgment, self-consciousness and effort. But since self-

consciousness is the most important of these and implies an advanced stage of the mental life with which the other proper differences are inseparably bound up, or of which they are only different aspects or expressions, I should prefer to treat the other differences in relation to self-consciousness as the focal point. Mr. Mezes indeed makes self-consciousness the fundamental difference, since he regards it as the condition of speech and judgment. It is admitted, he says, that animals use language, but man alone has the power of speech. What, then, is the difference between language and speech? The power of making judgments is the essence of speech, and self-consciousness is the essence of judgment (141). Mr. Mezes also says (142) that speech is made up of assertions, and that assertion is a self-conscious procedure. Hence animals not possessing self-consciousness are incapable of speech.

But when it is said that speech is 'an *attempt* or *endeavor* to convey by signs some idea of certain facts,' and that animals, though they may convey meaning, do not *intend* to do so (142), these statements are not only questionable, but they do not represent 'only the points of agreement' among authorities to which Professor Mezes claims to restrict himself in this connection. "Consider, for instance, the mere groans of pain of the solitary hunter accidentally shot in the forest, and compare with them the groans he gives later to tell an unskilled nurse that his position is uncomfortable. The first groans are significant enough, and would be understood by any one at hand, even the hunter's dog; they are in fact like the language cries of animals. The second groans assert discomfort, and are human speech" (141f.). The word 'assert' implies self-consciousness; but the latter term is not yet defined, and in the light of the illustration and definition of speech just given comparative psychologists would not agree in denying to animals the use of speech. A dog that is ill will sometimes lie quite quietly when no one is in the room, and then when its master comes in will utter groans apparently for the purpose of indicating that it is suffering and of evoking some expression of sympathy. When a dog with an injured foot simply goes about the house on three legs, this fact conveys a meaning; *we* obtain from the fact an idea which the dog does not intend to convey. But when the same or another dog comes directly to its master and holds up its wounded paw, I cannot doubt that this is an attempt to convey by a sign an idea of the fact that its foot is injured, and that the dog *intends* to convey this meaning. Or, take the illustration quoted in this connection (p. 139) from Romanes: "Terrier A being asleep in my house,

and terrier B lying on a wall outside, a strange dog, C, ran along below the wall on the public road following a dog-cart. Immediately on seeing C, B jumped off the wall, ran upstairs to where A was asleep, woke him up by poking him with his nose in a determined and suggestive manner, which A at once understood as a sign; he jumped over the wall, and pursued the dog C, although C was by that time far out of sight, around a bend in the road." Now if in this instance the dog B suggested to A only what we symbolize by the word 'Come,' as Lloyd Morgan holds, we have at least an illustration of what Morgan calls 'indicative intercommunication,' and what Romanes calls 'intentional sign-making,' which is common to animals and human beings.

It is not necessary to multiply illustrations; it is more important to observe that according to the view of the development of mental life expounded in Chapter VII., as well as in accordance with the opinion of competent observers generally, the child not only employs intentional sign language to express its wants and emotional states, but also begins to talk and uses phrases '*in order to communicate* information * * * to other minds' (Romanes) for some time prior to the rise of the consciousness of self. An animal or a child, in short, may *intentionally* convey meaning, may endeavor to convey some idea of certain facts; but this meaning is only the expression of the self of the moment; the child or the animal does not consciously refer its acts to itself as the permanent subject of them, nor can it reflect upon and analyze the conscious states which it may have had at the time it conveyed the idea of certain facts. Professor Mezes would agree that this reflective self-consciousness separates man from other animals, but as I understand him he would draw the dividing line lower down, at volition. And in summing up what has been said of the mental differences between man and brute, we are told that 'effort, the essence of volition, differences men from animals' (149). Here apparently we have the difference reduced to its lowest terms. We have already been told (65) that the feeling of effort is a sensation which is made up in part of sensations of movement and in part of cenesthetic sensations; but Mr. Mezes, of course, would not deny that animals have these sensations, though I think it would add to the clearness of his exposition if he expressly distinguished between muscular and volitional effort. When he says (145) that 'man can put forth effort, can try, can set his teeth and square his jaw to accomplish what he wills, while animals have no such power * * * ' we must lay the emphasis upon 'wills' and remember that 'to mean something and to intend or will it is the same' and that animals do not intend or mean anything.

This is not the place, even were I competent, to criticise Mr. Mezes's view of the rôle played by effort in human and animal life; but it may be noted that as most psychologists would agree with Romanes that animals intentionally convey meaning, so I believe they would agree with Lloyd Morgan that the dog which might have bitten him while he was sewing up its gashed side, but did not do so, possessed a kind of volition (perceptual) involving conscious effort, though not the capacity for 'introspective reflection in the light of self-consciousness.'

One other point may be mentioned in this connection. When Mr. Mezes says that 'animals possess general ideas,' it would be well to define what he means by general ideas. For if by general ideas he means 'abstract' ideas, Lewes and Mivart deny that animals possess them, while Wundt thinks the evidence is on the whole against it. Lloyd Morgan and Romanes draw the dividing line between man and brute in regard to the possession of abstract ideas at different places, the former denying, and the latter holding, that animals are capable of forming abstract ideas in the sense of what Principal Morgan calls 'conceptual isolates.' And even Romanes denies that animals possess general ideas in the sense of 'concepts,' since these are conditioned by the presence of that kind of self-consciousness which animals lack.

Part II., which deals with the *summum bonum*, must be more briefly treated. Men have always agreed that there is some one thing which is of supreme worth, and theories of this one supreme thing fall roughly into two classes, viz., eudæmonistic theories and perfectionistic theories (p. 190). Following Aristotle, Professor Mezes quite correctly identifies eudæmonism with welfare, and not with happiness in the purely hedonistic sense. But when he says that Aristotle's account has been long and much neglected, and that 'it is difficult to point out any one since Aristotle who has not been guilty of the confusion' between happiness and welfare (p. 398), one must demur. Moreover, since Professor Mezes expressly distinguishes eudæmonism from hedonism, what room is made for hedonism in the above rough classification? When he speaks of 'the difference between eudæmonists, perfectionists and believers in a complex end' (p. 193), he would seem to identify welfare with happiness and to overlook the fact that εὐδαιμονία is itself a complex end. Instead of saying 'the good is complex, being a combination of welfare and perfection,' I should prefer to say that perfection or virtue is the most important constituent of the complex end, welfare. Mr. Mezes describes his own theory of the good as 'sentient welfare'—by which he means the welfare of all sentient

beings. But I think the phrase unfortunate because ambiguous, since it might be taken to mean the welfare or good of the sentient nature of man, and would thus prove misleading.

Having determined that sentient welfare is the ultimate end, this, of course, becomes the leading criterion of objective morality. But there are also other criteria. There are a number of sources from which accounts of the trustworthy components of conscience, *i. e.*, such demands of conscience as make for objective morality, could be derived. But the doctrine of the cardinal virtues furnishes the most reliable and convenient account. 'A virtue is a quality of character that insures the performance of objectively moral conduct,' and since 'the doctrine of the cardinal virtue undertakes to give a full account of the perfectly moral character,' this furnishes the best foundation for a theory of objective morality (201, 202). The greater portion of Part II. is accordingly occupied with a discussion of the four cardinal virtues, to which the author adds benevolence. Mr. Mezes seems to me at his best in the kind of discussion for which these chapters afford opportunity. It is, of course, impossible to review them at length, but one or two points may be noticed by way of criticism.

In the chapter on Courage, Mr. Mezes says, 'the habit of moral courage is subjectively right and in so far approved, but is also at times injurious rather than helpful, and cannot, therefore, be called a virtue' (220). But if this be so, I do not see how veracity or even benevolence or justice can be called a virtue. Justice, benevolence, and veracity are qualities of character which are undoubtedly 'helpful' on the whole and tend to promote objectively moral conduct. But any of them may *at times* prove injurious to the larger interests of society, so far at least as our limited view can judge. It may, indeed, be contended that the good or evil consequences of an action —*i. e.*, its objective morality—are exactly proportionate to the good or evil in the motive, as T. H. Green would teach. But this view proceeds upon the supposition that moral conduct is measured in terms of perfection of character. Measured in any other way, good motives may always at times lead to injurious consequences, and to deny that moral courage is a virtue because it is at times injurious rather than helpful in its effects, seems to me to confuse two things which Professor Mezes is elsewhere careful to distinguish, viz., 'a quality of character with the conduct to which it leads' (p. 219).

The object of the chapter on Temperance is to discover and formulate the rules which should govern the appetites and desires, in

accordance with the demands of objective morality. The three fundamental and typical appetites of food, drink and sex, which are distinguished from the 'lesser desires' (p. 222), form the subject-matter of this chapter. An appetite is defined as 'a desire with a massive bodily basis,' and in the discussion the terms appetite and desire seem to be employed as synonymous. The failure to distinguish between appetite and desire accounts for the omission of what seems to me an important factor in explaining the use of intoxicants, viz., the influence of organic craving, which I should distinguish from the 'desire for heightened consciousness.'

The longest and most important chapters in Part II. are those on Benevolence and Justice. The ordinary meaning of the former word is extended, while that of the latter is usually, though not with thorough consistency, restricted to legal justice; with the result that some topics which would ordinarily be included under justice are treated under the head of benevolence. In Mr. Mezes' terminology benevolence 'includes all inclinations to objectively moral conduct.' But since it is easier to discover what deeds are allowed than it is to know what inclinations (or feelings) are approved, if we first discover what conduct is sanctioned, we may then infer what opinions are entertained towards the feelings that prompt it (265–8). Law thus becomes the criterion of objective benevolence; and since justice is the moral quality with which the law preeminently deals, it would seem to me more natural to discuss Hostility to External and Internal Public Enemies under the category of justice. Almost the entire subject-matter of the chapter on Benevolence is ordinarily dealt with in works on International and Private Law. When Mr. Mezes remarks that international law recognizes the principle that nations must have their 'quarrels just,' and proceeds to state the conditions precedent to a 'just war,' he is using the word 'just' in the moral and not the legal sense to which he by definition restricts it; but he is also recognizing the principle that justice and not benevolence is the basal concept in international law. Indeed he habitually speaks of benevolence where ordinary usage would suggest the word justice, as in the statement that 'benevolence has always required gratitude for benefits received' (296), and that the benevolence of gratitude rests on a basis of desert (299).

Professor Mezes defines justice as 'that portion of morality that consists of customs and usages that can be embodied in a code of laws,' and the just man as one 'who knows and lives up to this code.' Yet he does not identify moral and legal justice, as these

statements might seem to imply. He recognizes that systems of justice—*i. e.*, legal systems—do not embody ideal justice (303), and that the law and finding of the courts is not always just (314); but since aside from the courts there nowhere exists a sustained and coöperative effort to work out the idea of justice in detail, he holds that legal justice furnishes the best basis on which to found moral justice (209). I should say that the relation is precisely the other way. But when it is clearly recognized that legal justice is not to be taken as the measure of moral justice, that the latter frequently demands a modification of the existing code, and that there are 'moral actions the courts cannot consider and enforce,' no objection need be taken to Mr. Mezes's method for the purely descriptive purpose he has in view. Justice as embodied in codes of law furnishes a good basis for the description of kinds of conduct commonly regarded as just, though I do not see how justice so conceived, and with the limitations above indicated, 'may serve as an authoritative moral guide to all peoples and nations' (326). In his effort to make it serve this purpose, Mr. Mezes is in some danger of lowering the standard of moral justice, as, for example, when he says that the general legal and *moral* rule is *caveat emptor* (353). Nevertheless, the chapter on Justice, of which the author speaks with diffidence in the preface, is perhaps the most original, painstaking and thorough chapter in a book which covers a wide area and deserves careful study. It is no light work to have accomplished the task Mr. Mezes had set himself to do, with the success which he has achieved.

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NEW BOOKS.

The Varieties of Religious Experience. A Study in Human Nature, being the Gifford Lectures on Natural Religion delivered at Edinburgh in 1901–1902. WILLIAM JAMES. New York, Longmans, Green & Co. 1902. Pp. xii + 534.

Studies from the Yale Psychological Laboratory, edited by E. W. SCRIPTURE. Vol. IX. Researches on the Rhythm of Speech. New Haven, Yale University. Pp. 143.

University of Iowa Studies in Psychology, edited by GEORGE T. W. PATRICK. Iowa City, The University. 1902. Volume III. Pp. 144.

Die Analyse der Empfindungen und das Verhältniss des Physischen zum Psychischen. E. MACH. 3d Enlarged Edition. Jena, Fischer. 1902. Pp. ix + 286.

Ueber Annahmen. A. MEINONG. Leipzig, Barth. 1902. Pp. xv + 298.

Studien zur Werttheorie. ROBERT EISLER. Leipzig, Duncker & Humblot. 1902. Pp. xii + 112.

Principles of Western Civilization. BENJAMIN KIDD. New York and London, The Macmillan Company. 1902. Pp. vi + 538.

NOTES.

THE University of Edinburgh has conferred the degree of LL.D. on Professor William James and on President J. G. Schurman.

PRESIDENT NICHOLAS MURRAY BUTLER has received the degree of LL.D. from Johns Hopkins, Princeton, Pennsylvania and Yale Universities.

PRESIDENT F. L. PATTON has resigned the presidency of Princeton University, but retains the chair of ethics.

MISS MARGARET F. WASHBURN, Ph.D. (Cornell), has been elected to the professorship of psychology in the University of Cincinnati vacant by the removal of Professor Judd to Yale University. Miss Washburn is at present warden of the women's college at Cornell University and will occupy a similar position at Cincinnati.

HOWARD C. WARREN has been promoted to a full professorship of experimental psychology at Princeton University.

AT Cornell University, Dr. Ernest Albee and Dr. Albert Lefevre have been promoted to assistant professorships in philosophy, and Dr. I. M. Bentley to an assistant professorship in psychology.

A PSYCHOLOGICAL laboratory has been established at Mt. Holyoke College, five rooms on the top floor of Williston Hall having been assigned for this purpose. A substantial equipment of apparatus has been provided and enlargements are being rapidly made. The work is under the immediate charge of Dr. Thompson. Dr. Talbot is at the head of the department.

A BRANCH of the Psychological Association was established at Chicago on April 19, 1902. A committee consisting of Professor Coe, of Northwestern University, Professor Dexter, of the University of Illinois, and Professor Angell (chairman), of the University of Chicago, was appointed to arrange for a meeting during the coming year.